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HORIZONTAL CURRENTS
AND EASTATION

JOHN MARTIN





HORIZONTAL CURRENTS.



MARTIN'S THEORIES
OF
HORIZONTAL CURRENTS
IN THE
OCEAN AND ATMOSPHERE,
AND OF
EASTATION OF PLANETARY AND OTHER
CELESTIAL BODIES.

BEING NEW THEORIES OF NATURAL FORCES NOT BEFORE DISCOVERED,
DEMONSTRATING THE STABILITY OF THE SOLAR SYSTEM, AND
ACCOUNTING FOR MANY NATURAL PHENOMENA,
HITHERTO UNSOLVED PROBLEMS.

By JOHN MARTIN,

OF MELBOURNE, AUSTRALIA.



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MARTIN'S THEORIES

ON

HORIZONTAL CURRENTS.

PART I.

OCEANIC CIRCULATION.

OF the Gulf Stream, Captain Maury, in his interesting work, "The Physical Geography of the Sea," says :—

"There is a river in the ocean. In the severest drought it never fails, and in the mightiest floods it never overflows; its banks and its bottom are of cold water, while its current is of warm; it takes its rise in the Gulf of Mexico, and empties into Arctic seas; this mighty river is the Gulf Stream. There is in the world no other such majestic flow of waters. Its current is more rapid than the Mississippi or the Amazon, and its volume more than a thousand times greater. Its waters, as far out from the Gulf as the Carolina Coast, are of indigo blue. They are so distinctly marked that their line of junction with the common sea-water may be traced by the eye. Often one half of the vessel may be perceived floating in Gulf Stream water, while the other half is in common water of the sea. What is the cause of the Gulf Stream has always puzzled philosophers. Many are the theories and numerous the speculations that have been advanced with regard to it. Modern investigation and examination are beginning to throw some light upon the subject, though all is not yet entirely clear. But they seem to encourage the idea that this stream, as well as all the *constant* currents of the sea, is due *mainly* to the *constant* difference produced by temperature and saltness in the specific gravity of water in certain parts of the ocean. Such differ-

ence of specific gravity is inconsistent with aqueous equilibrium, and to maintain this equilibrium these great currents are set in motion."

This description conveys a good idea, at once, of the character of this and many other similar streams, and of the difficulties which beset the inquiry into their causes. Mr. A. G. Findlay, in his very valuable nautical work, "The North Atlantic Ocean," gives also a graphic description of this most prominent stream. He remarks—

"It was formerly held that the Gulf Stream flowed in a majestic current of warm water from its surface to its bed. The magnitude of its effects and the extent of its area seemed to leave room for no other conclusion. But the first observation of ice-cold water in its narrowest and strongest part overturned all these hypotheses, and left a most perplexing condition, in which we are at a loss to know where to look for an explanation. How this cold water, flowing directly contrary to the course of the upper strata, should preserve its polar characteristic almost unimpaired to such an enormous distance from its origin, and under such apparently adverse circumstances, cannot be explained with our present views. Again, by what power, or source of action, are the lower strata made to move in an opposite direction to those superincumbent upon it, and which, it might be supposed, would be acted on by the same laws and move in the same manner? It is one of the most remarkable evidences of that grand compensating system by which the Great Creator has commanded that all the harmonies of the universe shall be maintained—which keeps the atmosphere and ocean in a perpetual condition of interchange, and thus makes them fit for the sustenance of his creatures."

The Gulf Stream may be taken as typical of all the warm streams, of the oceans, flowing from the equator to the poles. And the cold counter-stream referred to by Findlay as underlying the Gulf Stream, may be taken as representative of all the streams, and they are many, from the poles to the equator.

It is clear, from the constancy with which both of these opposite currents flow, that each must be impelled by some

energy of its own, from whatever source that may be derived.

It having been observed that these streams and counter-streams are widely distributed, and prevail, in fact, in all parts of the oceans, it has been assumed that there must exist some perfect scheme of circulation under which they move. It is impossible that it could be otherwise than that such a system does exist, and many have, for ages past, been the attempts to unravel the mystery attending it.

The favourite theory at the present day is that a change of specific gravity through heat and saltness disturbs the equilibrium of gravitation, and that in ascending and in descending, horizontal currents are created by the displacement during the restoration of equilibrium. Whether or not horizontal streams and counter-streams thousands of miles long can be created by the effort of water to gain its level in so shallow a depth as that of the ocean, averaging three miles, is a matter which cannot be decided by actual experiment. Sir John Herschel felt very strongly the impossibility of it, and he held that the constant *winds* were the cause of the constant currents; but even that, if true, would not relieve us of the difficulty, for our inquiry would only be transferred from the ocean to the atmosphere, and our search for any theory whereby to explain a grand general scheme of atmospheric circulation, seems the much more difficult one of the two.

Captain Maury is a most elaborate expositor of the gravitation theory. He has had the advantage, and it must be admitted that he has made good use of his opportunities, of being able to tabulate the results of an immense number of nautical observations. His work on the "Physical Geography of the Sea" is now in its fifteenth edition, and is very exhaustive as regards his own theories. I am indebted to that work for a knowledge of many of the natural facts connected

with the ocean streams and their courses. In no other work is so much information on the subject to be found.

No attempt has yet been made by philosophers, so far as I can learn, to account for winds and ocean currents by any other force than the vertical one of gravitation. One after the other has admitted the insufficiency of the theory which they confess is only tenable until a better one be discovered. In the more distant planets, Saturn and Jupiter, the atmospheres are formed into regular but changing belts parallel to the equator. Heat and gravitation are again the only theory suggested for the phenomenon. Extending our observation to the sun we find similar belts. The belts there are not made visible to us in the same manner as those of the planets. The planets radiate reflected light, and but for the difference in the reflecting power of the clouds or atmosphere in the different parallels we should probably be unable to observe their belts at all. With the sun, it is different; he shines with no reflected light, but with his own. He has no greater heat, so far as we know, at his equator than elsewhere, yet belts on his surface are as unmistakable as those on that of the planets. The sun-spots, which have been aptly compared to the hurricanes of our atmosphere, appear in belts.

"They have been rarely seen in the equatorial region of the sun, between 3° north and 3° south latitudes, and they do not occur at all in the polar regions. They are, on the whole, most frequent in the region between 11° and 15° north of the equator."—Humboldt's "Cosmos."

Their region is estimated to extend, in northern and southern latitudes, to 29° by Galileo, and to 35° by Sir John Herschel. In rare cases they have been observed by Schwabe as high as 50° .

"This distribution of spots on the sun's disc, their rarity under the equator and in the polar regions, and their *parallel position* in

reference to the equator, led Sir John Herschel to conjecture that the obstructions which the third vaporous external atmosphere may present at some points to the liberation of heat, generates currents in the sun's atmosphere from the poles towards the equator, similar to those which upon the earth occasion the trade-winds and calms near the equator."—"Cosmos."

That *constant* currents to and from the equator and poles do prevail in the sun's atmosphere seems certain, yet nobody appears to have caught the idea of even searching for any *direct* horizontal energy by which horizontal currents are produced.

It would appear as if, with Sir Isaac Newton, all ambition in this field of abstract inventiveness had died out. During the last hundred years there has probably not lived one philosopher or physicist who has not felt that there is some link wanting in our theory of recognised natural forces, who has not known that there occur simple phenomena in nature which cannot be accounted for by the sole force of gravitation—who has not observed facts which appear inconsistent with the so-called Newtonian theory that all the movements of our planetary system are caused by the single law of gravitation. Yet all shrink from the responsibility of uttering, of even entertaining, a thought that there may have been some misinterpretation of Newton's language. A spell seems to have been, by a few summarising words of that great mathematician, limited in their reference strictly to the force and natural facts which had been the subject of his study—a spell seems to have been, by a few careless words of his, cast over all subsequent study of the subject, and I believe that an improper application of the infinitesimal calculus is much to blame for having placed an almost impenetrable barrier in the way of further research.

Take, for example, a quotation from an admirable lecture

of Dr. Tyndall. After demonstrating that the tides act as a constant brake, and "*must diminish the velocity* of the earth's rotation," he quietly disposes of the difficulty by adding, "the diminution is, however, too small to make itself felt within the period over which observations on the subject extend." That period of observation, I may add, embraces no less than two thousand years of *recorded* observations, and, taken with other brakes, one might expect a retardation to some extent, however small, to be felt. We shall not be able, in a similar way, to disregard the action of the brake when I have demonstrated that the rotation of the earth acts as a huge centrifugal mill, setting in motion with never flagging energy those enormous volumes of water whose stupendous movements have been for ages past, and still are, a subject of wonder and admiration, setting in motion not only those streams which by their velocity, position, and conspicuousness have attracted our attention, but also almost every particle of the whole bulk of water, of which the wide oceans are composed. We are approaching a time when it will be necessary to consider whether some force must not be in operation whereby to compensate for retarding agencies.

The total disregard of this important consideration—retardation, is readily traceable to a great faith in the correctness of the interpretation of Newton, by his successors. Everybody who has read a text-book, good, bad, or indifferent, on natural philosophy, must have met in one shape or another the assertion that "Newton traced to the single law of gravitation *all* the movements of our planetary system," as if nobody ever since had (as I believe nobody has) done it. Galileo, a few years before Newton's time, by observation and experiment, found that bodies fell to the ground under a certain law of acceleration. Newton caught the idea that bodies so falling did not fall in parallel lines but always to a



central point by a force of attraction, and that all matter throughout space was possessed of the same property of attraction, which he called gravity; further he supposed that all bodies attracted all other bodies in a degree depending on their mass and distance. But this was only a supposition, and before it could be promulgated as a fixed law it was necessary for Newton to show that the movements of planetary bodies in *the particular curves* of their orbits corresponded exactly to the curved path which would be laid down theoretically under his law of gravitation. Finding, in nature, the mean value of centrifugal force to be an unvarying quantity, he was justified in taking it as such, for the purpose in hand, without inquiring into its nature.

Taking the moon to experiment upon, he assumed that her orbit was circular, that the impulsive energy with which she moved was at a right angle to the line connecting her with the earth, and that the force of gravitation acted in lines practically parallel to each other in the small arc through which she moved in one minute of time. The labour of proving the correspondence between theory and nature was spread over a period of twenty to twenty-five years. In a circular orbit, he found that, although never actually approaching the earth, she would always be performing a movement which he assumed to be a fall, and the task he set himself was to demonstrate that the rate of acceleration in that movement corresponded with that which gravitation, as found on the surface of the earth, would produce at that distance. He found that on the basis of his calculation they did correspond, and, observing a similar rate of acceleration in all orbits, he (erroneously, as I think) assumed it to be the exact measure due to gravitation alone. Nobody seems to have since questioned the correctness of that assumption; I feel bound now to do so.

I left the ocean streams in Captain Maury's hands, and let us, with his assistance, learn something more of them, and of the Gulf Stream in particular.

"No feature" (he says) "of the Gulf Stream excites remark among seamen more frequently than the sharpness of its edges, particularly along its inner borders. There it is a streak in the water. As high up as the Carolinas this streak may be seen, like a greenish edging to a blue border—the bright indigo of the tropical contrasting finely with the dirty green of the littoral waters. It is this apparent reluctance of the warm waters of the stream to mix with the cool of the ocean that excites wonder and calls forth remark.

"As to the temperature of the Gulf Stream, there is, in a winter's day, off Hattaras, and even as high up as the Banks of Newfoundland in mid-ocean, a difference between its waters and those of the ocean near by of 20° and even 30°."

Whatever be the nature of the motive power, and whether this operate vertically or horizontally, or both, it is evident that change of density is what destroys equilibrium.

The passages cited, show that on either side of a well-defined boundary, there exist two waters possessing different density, a condition under which the lighter should rise and spread over the denser, and the denser sink and spread under the lighter. But what do we see? The lighter water, before it can rise and overflow in the direction which should be its path of least resistance, is borne forcibly away from it by some unknown superior force, and carried off before the vertical force can overcome the inertia of the mass. Surely if gravitation cause an overflow through difference of density, at a distance of thousands of miles between the equator and the poles, how much more decidedly and quickly should it operate where the waters with a similar difference of density, are brought to within a mile or even within a few yards of each other.

Apparently this difficulty forced itself on the mind of Sir

John Herschel, and led him into another extreme when he wrote (*Encyc. Brit.*) as follows :—

“The dynamics of the Gulf Stream have of late, in the work of Lieut. Maury, already mentioned, been made the subject of much (we cannot but think misplaced) wonder, *as if there could be any possible ground for doubting* that it owes its origin *entirely* to the trade-winds,” and “If there were no atmosphere, there would be no Gulf Stream, or any other considerable oceanic current (as distinguished from a mere surface drift) whatever.”

In the eyes of Sir John Herschel, Maury's theory must indeed have appeared utterly absurd, or he could scarcely have been led into uttering so strong a condemnation of it. Maury, in a subsequent edition of his work, successfully turns the tables on Sir John, by proving that the trade winds can have very little influence on the currents, inasmuch as those currents do not flow with the wind. He says—

“This (Herschel's) theory would require all the currents of the sea to set with the winds, or when deflected, to be deflected from the shore, as billiard balls are from the cushions of the table, making the littoral angles of incidence and reflection equal. Now, so far from this being the case, *not ONE* of the *constant* currents of the sea makes such a rebound or sets with the wind.”

He shows further that many of those currents “run right in the *wind's eye*.”

Of the two theories, that of the winds is certainly the weaker. But even if that were the correct one the question would still arise—by what force are the *constant winds* urged in their horizontal course of thousands of miles at a stretch?

The atmosphere, like the ocean, is liquid—free under any pressure to move in the path of least resistance. If there be any known or unknown force by the mechanical application of which horizontal currents are produced in the ocean in virtue of its fluid condition, it is likely to follow that the same force will equally produce similar currents in the equally

fluid atmosphere, and that the currents so produced in the ocean and in the atmosphere will be, in the main, quite independent of each other, but quite similar in character and direction, and if we could, in a revolving sphere, find a familiar mechanical application of a known force which would necessarily impart to fluids of different density a horizontal motion to and from the poles and the equator, we should have solved that difficult problem, "Whence cometh the winds and whither they goeth;" we should have found one general scheme of circulation which would produce at once a regular wind and a regular ocean current.

I will presently show how such a mechanical application does exist, and how it is almost impossible for any portion of our ocean or of our atmosphere to remain under existing conditions in a state of rest.

The Gulf Stream flows from the south; its fountain-head may be said to be in the Gulf of Mexico, in the shallows of which the water is heated by solar rays; thence it skirts the American coast; from Nova Scotia its course runs east, and breaking on the coast of Europe, it pursues a northward course, and loses itself in the Arctic regions.

Such a huge mass of water cannot move in one direction without an equal quantity moving in the opposite, hence we find there to be return currents which are also constant from the poles to the equator. But the warmer streams are always more conspicuously presented to our notice, because, by their superior buoyancy they maintain their character as *surface* streams. Of cold under-currents our knowledge is more limited.

It is a singular feature in the Gulf Stream, that by certain constraints put upon its course, it flows in a direct line from the coast of America to that of Europe, bridging over *all* the cold currents which flow, in constant courses, underneath and constitute the cold streams of the North Atlantic Ocean.

Similar systems of currents to those of the North Atlantic are found in all the other oceans. The cold streams come up from the polar regions, and meet at the equator, the stream from each side opposing that from the other as a barrier and a wall.

All the warmer surface streams along the equator flow from east to west, and from the coast on which that stream impinges the chief warm current of each ocean starts on its polar journey. There is no exception to this rule. One law evidently controls all.

The chief currents, up and down, uniformly describe a circuit and enclose a kind of (at least as regards the surface water) neutral ground, where the water, being of a medium heat, has no visible tendency in any direction. This quiescent state and the uniform temperature of the water here are favourable to the growth of sea-weed. The area in which the Gulf weed luxuriates is, in the North Atlantic, called the Sargasso Sea.

“Covering an area equal in extent to the Mississippi Valley, it is to thickly matted over with Gulf weed, that the speed of vessels passing through it is often retarded. When the companions of Columbus saw it, they thought it marked the limits of navigation, and became alarmed. To the eye, at a little distance, it seems substantial enough to walk upon.” (Maury.)

A Sargasso Sea, more or less conspicuous, is formed in each ocean, and is bounded by the circling system of currents. In accounting for the system of circuits, Captain Maury elaborates the subject of the causes of change of density, but he traces all dynamic effect to gravitation only. He says that change of temperature and change of saltness upset the vertical equilibrium, and that the restoration of the equilibrium necessitates a change of position, which is undeniable. But the movements that would be produced by such conditions, would be only a *radiating* overflow and underflow.

Although such a radiating overflow does take place, the movement is only a secondary one, and it could not assume the shape and character of a "*River in the Sea*." For example, take the Gulf Stream. Its breadth in the "Narrows" of the Straits of Florida is thirty-two miles. By a lateral overflow the breadth of the defined stream is increased to seventy-five miles off Hatteras, and that overflow is the result of the difference of *specific gravity* referred to by Maury. But in the time that, under this force, the water has spread forty-three miles, the whole mass of it has travelled *bodily* nearly 700 miles to the north. In fact, the warmer water is carried down stream so much faster than it overflows, that on any one spot the overflow is neutralised and the position of the boundary between the waters of the stream and those on either side remains stationary and further overflow ceases. This absence of radiating overflow cannot be from want of difference of temperature because Maury tells us that the difference is here 20° and even 30° between the waters which are close together. The currents have *no where* the movement of a radiating overflow. Nor do the cold streams, as Maury's theory would require, sink and spread. The cold stream which debouches from Davis Straits impinges on the border of the warm stream, pushing its boundary line many miles to the south, then dips and flows under and across the warm one. The conditions of gravity seem to be satisfied here, but they are soon again disturbed. Having passed and rid themselves of the restraint of the superincumbent warmer waters, such a portion of the colder ones as had not been turned to the right as presently described bounds again to the surface against the pull of gravity, and, as a surface stream pursues its impetuous course towards the equator. It might, perhaps, be said that its momentum carries it on its course, but that only brings us again to the original point at issue. Whence is the

momentum derived? If the horizontal movement were only a result of the downward pull of gravity, nothing would prevent the warmer water from flowing, as a surface current, into Davis Straits.

Maury explains very fully, how, when, and where oceanic water becomes warmer or saltier. He attributes important dynamic effect to the saltiness acquired in the equatorial regions, and even says :—

“ Were it not for its salts, sea water, once near the surface within the tropics would, by reason of its warmth, and thermal dilatation, remain near the surface. Vertical circulation would be confined to polar seas, and many of the living creatures that inhabit its waters would perish for the lack of currents to convey them their food.”

My conclusion is quite the reverse. The motive energy of equatorial waters by virtue of their lightness through warmth is impaired by saltiness, and if there were no saltiness, the water would be so much lighter and have so much more energy. In the Gulf of Mexico (the head of the Gulf Stream, and hence its name) the water is heated, and escapes before very much evaporation takes place, hence the balance is in favour of the warmth, but if the water were retained there much longer it would become saltier and heavier until it became as heavy as when it entered the gulf, then there would indeed be no Gulf Stream. If the water were retained in the heating basin longer still, it would through saltiness become heavier than on its entrance, and a tendency to the equator, instead of from it, would of necessity result.

Such a phenomenon we do in fact see occurring constantly in the Red Sea, where the water is kept in a great evaporating pan so long that it becomes intensely salt, and very dense.

The *Red Sea* is a narrow strip of water a thousand miles long with a direction nearly north and south, and having only one mouth, which lies at the end towards the equator, and

is situated in latitude 13° north. This sea is in a district of burning sands; the heat suffered in it by travellers is proverbial, and the evaporation from its surface enormous, the result being that the water becomes extremely salt.

The currents at the mouth are strong—the surface one sets in, the under one out. The under one is, of course, the heavier, and that is the salter *and the warmer*. So we have here the warmer water heavier than the colder, and urged impetuously in a direction exactly opposite to that of the warmer water of the Gulf Stream.

The superior dynamical effect is produced in the Red Sea by excess of saltiness, in the Gulf Stream by excess of warmth. In neither case do the conditions assist, but in both they oppose, each other.

On all parts of the surface of the globe it is only horizontally that water can move to any considerable distance without constraint. All vertical movements are limited on an average to three or four miles, consequently such movements are not only short when made, but very slow, for the inertia of large masses of water takes time to overcome, and there is insufficient distance to admit of acceleration taking much effect. In horizontal movements it is different; the water may move in one direction for thousands and thousands of miles, and the laws of acceleration may produce currents with a velocity quite impossible under the vertical conditions. Urged by any horizontal force water is under but little constraint—not so when by a vertical force. Upwards the atmosphere at water level is the extreme limit, downward the solid bottom; between these limits again gravity may, by differences of density, impose other barriers and constraints; a lighter water will lie passively over a heavier, and each can, under a horizontal force, move freely within its own level without penetrating that of the other if the force applied be not too violent. In such case it will move as

between two guides which constrain its movements. If a force be applied to any point of water in a direction parallel to the guides, the whole force will take effect in producing motion, but if it be applied at an angle, part of the force will be lost by the resistance of the guide or restraining condition, whilst the remainder will take effect, and the value of so much as takes effect can readily be ascertained by decomposing it into two single forces.

A particle of water can move in only one direction at any given time, therefore when moving under two or more forces it must partake somewhat of each of them, and so much of each in proportion to the relative intensity of it. The intensity of a force is easily represented by a line. As the length of the line, so the intensity of the force. So, when, in a geometrical demonstration, it is found by the shortening of a line, that motion in a parallel direction has been lost, it may be taken as *proof* that the force moving the body in that *parallel direction* has met with resistance, and suffered a loss of its intensity. This law is one of the fundamental principles recognised in the science of mechanics, and upon the truth of it I shall, in the second part of this treatise, depend in some measure for the correctness of some most important deductions, just as, upon it, depends the correctness of the universally recognised law of the "*parallelogram of forces*," as enunciated in the following law, which, for the sake of correctness, I quote from a very recent work (Deschamel, 1873).

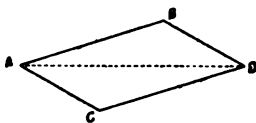


Fig. 1. Parallelogram of forces.

"If a material point, A, is acted on by two forces, represented in magnitude and direction by A B and A C, there is a resultant, which is *exactly* represented by the diagonal A D of the parallelogram of which A B and A C are sides."

Let us now apply this law to a particle of water in the ocean, and floating near the surface.

In figure 2, let AB be a parallel guide (water surface, for example), and E the particle of water to be operated on. Suppose this particle to be struck by a force from C which would, in a given time, and but for the resistance of the guide, have carried it to P .

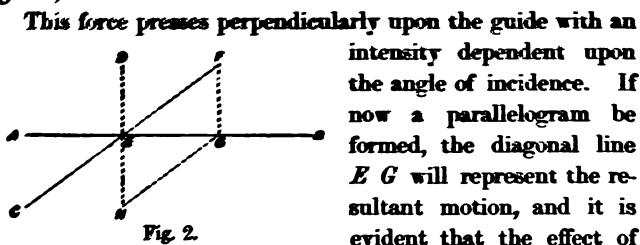


Fig. 2.

This force presses perpendicularly upon the guide with an intensity dependent upon the angle of incidence. If now a parallelogram be formed, the diagonal line EG will represent the resultant motion, and it is evident that the effect of the constraint of the guide, action and reaction being equal, is the same as if, simultaneously with the blow or impulse from C , the particle had been struck by another blow from D , which would have moved it to H in the beforesaid given time. In this illustration, the effect of the resistance of the surrounding medium is, of course, not taken into consideration in the diagonal produced.

We will next suppose the circle $NES E'$ (fig. 3) to represent a polar section of our earth: N and S , the poles; E and E' , the equator; NS , the axis; and N, f, g, h, E, j, k, l , and S , points respectively equidistant from each other. And suppose the axis to be capable of exerting perpendicularly to itself, and from all its parts, a repulsive force. Any particle of water at each of the points f, g, h, j, k, l , and also at i and m , would be struck by such a force from the respective points c, d, e, o, p, q, n , and r . Taking the horizontal line of the water as a constraining guide, and supposing the force to be equal in intensity at each of the points f, g , and h , and such force to be represented by the equal lines f, f'

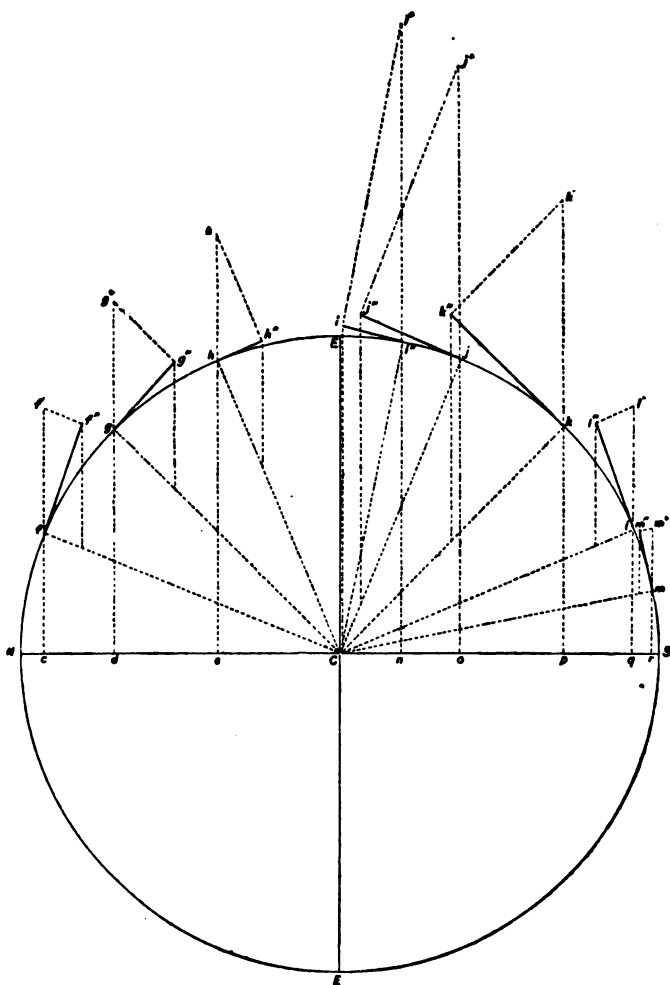


Fig. 3.

g, g' , and h, h' , then by the law of the parallelogram of forces, we find that the resultant motion will vary with the angle of incidence, and will be represented by the tangential lines f, f'', g, g' , and h, h'' . It will be noticed that nearer the pole the proportion left available for horizontal effect is more, and nearer the equator less. *There is in nature such a repulsive force* from all parts of the axis of the earth. It is generated by the earth's rotation, and is called centrifugal force. But it is not of equal intensity at points in different latitudes, because the length of radius is one of the measures of its intensity. As the time of rotation is the same at all points, we can indicate by the lines $i, i', j, j', k, k', l, l'$, and m, m' (equal to the radii), the comparative intensity of the force at each of the points i, j, k, l , and m , and the intensity of the force left available horizontally is shown by the diagonals $i, i'', j, j'', k, k'', l, l''$, and m, m'' . It will be seen that the intensity is greatest in about the 45th degree of latitude, and that it decreases towards the equator and towards the poles. At the poles, there is no centrifugal force, because the radius diminishes there to nothing, whilst at the equator, where the centrifugal force is greatest, it is neutralised by its striking the constraining guide at a right angle in all directions.

I have assumed this experiment to be made in the waters of the ocean, but it can be equally well applied to the atmosphere, in which the elasticity of the element is the chief cause productive of differences of density, whilst in the ocean heat and saltness are the great disturbing elements.

The figure of the earth is a spheroid, and the water-level is geometrically about 13 miles higher at the equator than at either of the poles, the equatorial diameter of the earth being about 26 miles greater than the polar. From the pole then, to the equator, a ship would sail up the slope of the mountain to a height of 13 miles, and continuing its course

across the equator would sail 13 miles down again. This slope I have called a geometrical one, because, in nature, the level is that of the equilibrium of the two forces—gravitation and centrifugal force. It is the length of radius, and the angle at which the two forces meet, which determines the level of the equilibrium, and the figure of the spheroid. The greater the velocity of rotation the more will matter be heaped up towards, and on, the equator. The major planets, Jupiter and Saturn, make one revolution in less than half the time taken by our earth, consequently we see much greater protuberances round the equator of those planets. The velocity with which a particle on Jupiter's equator revolves is enormous, and the mountain ridge round the equator is raised to and maintained at a height of about one-fifth of his semidiameter, or about 3,000 geographical miles high! The rotation of the planet does the work—does it constantly—of holding up this mountain against the *elastic pull* of gravitation. Physicists perceive no perceptible retardation in the axial rotation of a planet, they therefore conclude that the momentum or rotatory energy meets with no resistance, and for the same reason, they conclude that, in a planetary orbit, the projectile impulse meets with none. I think I shall show reason for differing from both conclusions, and I believe that the work is a tax upon the impulse or impulsive force. This question will be taken up further on. At all events, few will be inclined to think that such vast bodies of water as are contained in the ocean currents can have their inertia overcome, and be set in motion in a horizontal direction, as we find in nature, without a constant expenditure of the earth's rotatory energy, if it be found that they are set in motion by it.

I have demonstrated that deflected centrifugal force urges *all* particles of water from the poles to the equator. It re-

mains for me to show how some of those particles, and which of them, are urged with more energy than others, and to explain how lighter waters—the Gulf Stream, for example—move impetuously in a direction exactly opposite to the actual direction of the energetic force. Some familiar experiments will assist me in doing this.

In the ocean, and in a dish of water alike, gravitation determines the water-level, and acts as a horizontal constraint and guide.

Let us take some sawdust (previously saturated with water, so that, although it floats when the water is quiescent, it is not too light to sink on stirring), and throw it, mixed with an equal quantity of sand, into a dish of water placed on a revolving table. Both sand and sawdust sink; but mark this, that when revolving, the sand at the bottom flies to the *outer* edge of the dish, whilst the sawdust floating collects in the *centre*. Each grain of sand and of sawdust moves, not by the action of gravitation, or of a current, but by its own individual force. Here we have a simple yet exact representation of ocean currents.

In both cases—in the dish and in the ocean—gravitation acts downwards, and centrifugal force horizontally; centrifugal force in both cases radiating from a centre. In both cases the water is heaped up away from the centre, in both its surface level is depressed at the axis. In both the water is pressed outwards against a boundary wall, that wall being in one case the rim of the dish; in the other, the pressure, at the equator, of water from the other hemisphere, moving under similar conditions. In both we have the heavier particles, the lighter particles, and the fluid of medium density; the heavier particles—in the dish the sand, in the ocean the colder or the saltier water—are thrown outwards from the centre; and the lighter—in the dish the sawdust, in the ocean the warmer or the less salt water—are

pressed back to the centre ; the water of medium density taking up a kind of neutral position.

All that is required to make the analogy complete is to provide a furnace at the edge of the dish and a refrigerator in the middle of it, like the solar heat at the equator, and the want of it (cold) at the poles, the furnace having the power of turning the sand into sawdust, and the refrigerator of turning the sawdust again into sand. Even without a diagram my reader will readily see that the same force which made the sand and the sawdust move in their several directions, in the first instance, will, with such a transforming apparatus, urge them constantly to turn back on reaching the boundary, and that a constant circuit to and from the centre will be created. A constant outward flow of sand at the bottom of the water, and a return flow of sawdust at the top, will set in and continue, just as, in nature, we see the constant circuit of hot and cold water to and from the poles. Raising some parts of the bottom of the dish in imitation of the continents of the earth would further assimilate the movements.

I must here digress to draw attention to the nature of another measure of centrifugal force. I have already stated two of those measures to be radius and velocity ; the third is weight. All other conditions being similar and equal, the energy of centrifugal effect is as the weight. One pound will have, for example, half the effect of two pounds. Thus, I take a sphere measuring one cubic foot, and weighing one pound ; I tie it by a string one yard long to a point of an axle, and set the whole revolving at a certain velocity, which will create a pull of, say, one pound weight upon the point. If I take another similar sphere, and fix it in a similar manner to another point of the same revolving axle, I obtain a similar pull of one pound upon that point. If I now cut one of the strings, the centrifugal energy is transferred from

a *pull* upon the axle to a *push* of one pound weight upon any constraining point or area of resisting medium that may be met. If before cutting the string I had fixed the two spheres rigidly together, and then cut that one string, the pull of both spheres, equal together to two pounds, would be felt on the *one* point. If I now cut the second string, the two spheres will give a pressure of two pounds on the two points, or on the two areas of resisting medium; but if, before cutting the second string, I had compressed the two spheres of one foot each into one sphere of one foot, the push that would have fallen upon the two points or two areas of resisting medium will fall on only one, and the pressure upon that one will consequently *be doubled*. Density means weight relatively to bulk—the one foot sphere of two pounds weight is more dense than the one foot sphere of one pound. Now if, in my revolving pan, *emptied of its contents*, I place a sphere of water, or of a substance of exactly similar weight, say of wood, the sphere, in turning with the dish, will press outwards, and move away from the centre. But if the dish be filled with water, both the water and the sphere, having equal density, will press outwards with equal energy, radius for radius; the sphere will be unable to move outwards without displacing and *pushing back* an equal bulk of water, the two will therefore equilibrate each other, and no motion will ensue. But if, for a moment, the sphere became denser, its increased centrifugal force would enable it to move forward and press back into the place it had just vacated an equal measure of lighter water. That backward movement is the return stream, and if there had been in the dish a second sphere of water made lighter at the same moment that the first was made heavier, the lighter sphere, having least centrifugal energy, would be the first to retreat, for it would be pressed back by the bulk of water as well as by the heavier sphere. So exactly is it in

the ocean ; the colder water from the pole, by virtue of its density, has a superior centrifugal force, and it moves forward ; pressure is felt upon the neighbouring fluid before displacement takes effect, and those particles which offer least resistance are the first to move back.

This principle is clearly demonstrated in the North Atlantic Ocean. The polar water has superior energy, and every particle of it is, by an independent energy of its own, impelled towards the equator. The water of the Gulf of Mexico is warmer and lighter than any other in the ocean ; it possesses less centrifugal energy ; it offers less resistance to the displacing energy of its neighbours and rivals ; and against the tendency of its own inherent energy it beats an unwilling retreat. We thus strip the Gulf Stream of that exhibition of *superior* power with which it has been so romantically invested. Its vital energy is affected, though not destroyed, by its loss of density ; it retires towards the centre of motion, there to leave its energy regenerated, and its power restored by the abstraction of its heat. In the ocean, as in the dish, there is room only for a certain quantity of water ; if one mass of water advance, a similar one must retreat, and the retreating one must be that one offering the lesser resistance to the displacing power of the other. And so it is in all the oceans ; every particle of water is endowed with a living energy, pushing and struggling *against* its neighbour to get further and further away from the poles, gaining or losing ground, according to its relative density.

We can now understand how it may be that water, warmer or saltier than that surrounding it, may be at one time carried energetically away from the equator, at another time towards it.

What a different effect would have been produced if the mouth of the Red Sea had been directed *away* from the

equator, instead of *towards* it, for instead of salting up being prevented in it, it would have been facilitated, as the very power which urges the salt by its density out of the basin of the sea would, under the opposite condition, prevent its escape. It would be thus possible to turn the Red Sea into a gigantic salt-bed, if we could reverse its condition, by removing the isthmus to the southern end of the sea and the mouth to the northern.

A tropical region and a rainless district are, of course, conditions always necessary to the formation of salt-beds in this wise.

"Bifurcations" of the Gulf Stream at its borders are pointed out by Maury. He argues that these are not forks of the Gulf Stream, as "persons have been led to suppose," and he says—

"Now, if any vessel will take up her position a little to the northward of Bermuda, and steering thence for the Capes of Virginia, will try the water-thermometer, all the way at short intervals, she will find its readings to be now higher, now lower; and the observer will discover that he has been crossing streak after streak of warm and cool water in regular alternations. He will then cease to regard them as bifurcations of the Gulf Stream, and view them rather in the light of thermal streaks of water which have, in the plan of oceanic circulation and in the system of unequal heating and cooling, been brought together."

There need be little doubt about the cause of these streaks. Bodies of colder water have been, by local disturbance, brought temporarily to the surface, where they *must*, both by their impetus and their superior centrifugal power, continue the course which they pursued before rising from the bottom, that course being opposite to that of the warm water stream. If the disturbing influence be permanent, such as a projecting rock, the consequent streak will be a permanent one. Yet these streaks cannot be of any great length, because, as they originate in a disturbance of vertical equilibrium, they must

disappear with its restoration, which cannot be long in being effected.

So far have philosophers been from finding any cause of permanency in ocean currents, that even Maury admits its existence to be mere conjecture. He says :—

“We mainly rely upon hypothesis or conjecture for the assertion that there is a set of currents in the sea by which its waters are conveyed from place to place with regularity, certainty, and order.”



Fig. 4.—The Gulf Stream.

There are in the Gulf Stream some noteworthy features which admit of ready explanation by the theory of deflected centrifugal force.

The polar water, rushing through Davis Straits, meets that

of the Gulf Stream ; they both rush impetuously into each other's track ; yet no confusion ensues—they do not mix—each takes the path assigned to it by gravity ; the heavier sinks, the lighter floats. Still there must be some force whose requirements are not satisfied, for some of the polar waters, after dipping and passing under the warmer stream, rise again to the surface against the tendency of gravitation, and continue their blind career to the south in utter disregard of the lighter water left behind them. That force is of course deflected centrifugal force, by which every particle is endowed with a weight horizontally, according to its density, and quite distinct from its vertical weight through gravity.

At the place of crossing, the Gulf Stream is, by the momentum of the stream from Davis Straits, pressed from its north-easterly into an easterly course, and so its boundary line, after the obstruction is passed, turns again to the north, forming a curve which Maury likens to a horseshoe.

At this crossing the direction of the warm stream may be traced to be compounded out of at least four distinct horizontal forces or influences :—first, its own backward movement from the equator, through deficiency of centrifugal energy ; second (consequent upon the first), an easterly movement by passing into circles of smaller radii, with the velocity and momentum of larger circles in lower latitudes, an effect known as Dove's rotary law ; third, its own momentum from stored-up previously exerted and compounded forces ; and fourth, the pressure from the north-west through the momentum of the stream debouching from Davis Straits.

It is very clear that the horizontal movements of these waters can be but very little affected by their tendency to rise or fall through gravity.

In all oceans the sandbanks and mud bottoms must be formed or moulded by the currents which flow over them.

Of all oceans, the physical formation of the bottom of the Atlantic is the better known to us, because of the larger traffic, and consequently more numerous soundings that have been taken there.

The large volume of water entering the North Atlantic through Davis Straits does not underlie any warm layer, so we may fairly conclude it to be colder than the other cold currents which, on entering it, under-run the Gulf Stream, and get their temperature thereby raised. The Davis Straits water being then the colder, would be expected to cut the deeper channel.

Let us consider what course the Davis Straits water would naturally pursue. On entering the Straits, if it were not for the constraint of the coast line, it would take a direction to the south-west, in obedience to its superior deflected centrifugal energy, modified by the rotary law. The coast line, however, interferes; the westerly tendency is overcome, and, under its constraint, the stream tends to the south-east. It is not freed from the constraint till Newfoundland is passed.

It then impinges on the side of the Gulf Stream, which it meets, and which by its momentum it presses back, as before described. The tendency to turn then to the westward under the rotary law is assisted by the check of the opposing wall, and a turn downwards and to the right takes place. Before that turn downwards and to the right can be effected, the coast of Newfoundland will be passed by some considerable distance, and in the angle so formed there will be comparatively still water or an eddy. In this still water or eddy sand and mud should be deposited, and form a bank with shallow water over it; and here, in nature, a bank known as the Grand Bank of Newfoundland has, in fact, been formed. The downward impetus obtained by its dipping under the cross-current, aided by its own superior density, would cause

it to scoop out, with unusual vigour, all loose and soft matter from the bottom, and here we do find the very deepest water. Nowhere else in this ocean do the soundings show a depth of 4,000 fathoms; but here they are not only over 4,000, but over 5,000—they are recorded as 5,200 and 6,600. They are, however, unreliable, on account of the difficulty attending measurements at such great depths; but whatever the correct depth may be, it is certain that the deepest part of the ocean is found here. From this point the current would be allowed to follow its normal direction along the coast, which runs to the south-west, until turned again to the eastward by the reefs of the West Indies, after which the coast of Brazil presents opposition to the law of rotation, and the current would be compelled to hug that coast in its course to the equator. All this happens: excepting at the spot before mentioned, where the bottom is exposed to an extra scooping process, the lowest floor of the ocean is found where this coldest water cuts its channel. Here all the soundings are over 3,000 fathoms, excepting in one place, where it is crossed by a ridge, on which it is 2,800 fathoms, and that ridge seems to be a continuation of the ridge of the West India Islands. Elsewhere in the North Atlantic the soundings are under 3,000 fathoms. It has been considered remarkable that the equatorial current from east to west, after passing the Brazils, should strike from its apparently direct course, and pass tortuously through narrow gorges and over shallows through the Caribbean Sea and the Gulf of Mexico before debouching, a rushing torrent, through the Straits of Florida, as the fountain-head of the Gulf Stream. There appear to be two reasons for this apparently strange course. First and chiefly: In the shallows of the Gulf of Mexico the waters attain a higher degree of heat than elsewhere (Maury tells us they are 35° to 40° hotter on issuing from the Gulf of Mexico than on entering the Caribbean Sea);



hence they have a minimum centrifugal energy, and are the first to beat a backward retreat before their more energetic rivals for position ; their movement to the north, through the Straits of Florida, relieves the pressure of water from the Caribbean Sea, and in a chain of connection the water is drawn by diminished pressure into that sea from the main body of equatorial water. Secondly, just before entering those straits and shallows the cold stream, flowing in a counter direction, is, by the ridge just mentioned, directed upwards from a depth of 3,500 fathoms to a depth of only 2,800 fathoms, being a sudden rise in the ground of 4,200 feet ; this projecting upwards into and against the warm and opposite current, naturally assists to turn that current into the narrows between the islands, which direction it in the main would have had a tendency, in any case, to take.

The peculiarities of the forces in play, and of the effect of local circumstances and physical conditions, in the North Atlantic Ocean, may fairly be considered to represent similar ones in all the other oceans. The circuit of ocean currents is not performed in an endless band round the earth, in which case there might be less resistance to the rotation of the earth. All the cold currents are projected towards the equator, where they meet and spend momentum on each other and on the coast boundaries. Energy of motion so lost can only be recovered by future efforts of the earth's rotation. It always has been recovered, for the streams come up to the equator with renewed vigour, again and again with undiminished energy. The loss of momentum continues ceaselessly during every moment of time, and when we consider that the ocean waters amount to as much as $\frac{1}{1100}$ th of the whole mass of the earth, we cannot but conclude that it must have a very serious retarding influence. Every angular turn in a stream—every projecting rock or inequality of the bottom—the friction of surfaces in the

flow of one stream over another, and various other conditions which will suggest themselves, are all evidence of loss of energy and motion. Such conditions are found in abundance, especially in the Caribbean Sea and the Gulf of Mexico, in which the currents, as in all polar streams, are directed against the earth's rotation.

PART II.

ATMOSPHERIC CIRCULATION.

IF Maury found it difficult to find any law by which regularity, certainty, and order are maintained in sets of currents in the sea, how much more difficult would it seem to find a law governing those in the atmosphere, where there is an absence of his prime condition—saltness? The same simple law, however, which I have shown to govern the horizontal movements of waters of different density in the ocean, will, of course, equally govern masses of air of different density in the atmosphere. But both the nature and condition of water and air are different; the nature, because water is almost inelastic, whilst atmospheric air is extremely elastic; the condition, because the surface level of the water is clearly defined by its junction with the atmosphere, whilst the air has no clearly defined surface level, and because the water is subdivided by coast lines, whilst the atmosphere is free from any such constraint.

The most elaborate dissertation to be found on the theories of atmospheric circulation is, again, undoubtedly that of Captain Maury, but a good culling from previous writers is contained in Everett's translation of Deschamel's "Natural Philosophy," 1873.

As might be expected from a writer of high scientific attainments, extravagant and untenable theories are eschewed by Deschamel, so far as is compatible with having any theory at all; yet what is left seems to me unphilosophical enough,

and is meagre in the extreme for a subject so wide, and exhibiting such an immense display of dynamic force.

I will quote the article on General Circulation in full:—

Trade Winds. General Atmospheric Circulation.—The trade-winds are winds which blow constantly from a north-easterly quarter over a *zone* of the northern hemisphere, extending from a little north of the Tropic of Cancer to within 4 or 10 degrees of the equator; and from a south-easterly quarter over a zone of the southern hemisphere, extending from about the Tropic of Capricorn to the equator. Their limits vary slightly according to the time of year, changing in the same direction as the sun's declination. Between them is a zone some 5° or 6° wide, over which calms and variable winds prevail.

"The cause of the trade-winds was first correctly indicated by Hadley. The greater power of the sun over the equatorial regions causes continual ascent of heated air from them. This flows over to both sides in the upper regions of the atmosphere, and its place is supplied by colder air flowing in from both sides below. If the earth were at rest, we should then have a north wind sweeping over the earth's surface on the northern side of the equatorial regions, and a south wind on the southern side. But, in virtue of the earth's rotation, all points on the earth's surface are moving from west to east with velocities proportional to their distances from the earth's axis. This velocity is nothing at the poles, and increases in approaching the equator. Hence, if a body on the earth's surface, and originally at rest relatively to the earth, be urged by a force acting along a meridian, it will not move along a meridian, but will outrun the earth, or fall behind it, according as its original rotational velocity was greater or less than those of the places to which it comes. That is to say, it will have a relative motion from the west if it be approaching the pole, and from the east if it be approaching the equator.

"This would be true, even if the body merely tended to keep its original rotational velocity unchanged, and the reasoning becomes still more forcible when we apply the principle of conservation of angular momentum in virtue of which the body tends to increase its absolute rotational velocity in approaching the pole, and to diminish it in approaching the equator.

"Thus the currents of air which flow in from both sides to the equatorial regions, do not blow from due north and due south, but from north-east and south-east. There can be little doubt that, notwithstanding the variable character of the winds in the temperate

and frigid zones, there is, upon the whole, a continual interchange of air between them and the intertropical regions, brought about by the permanent excess of temperature of the latter. Such an interchange, when considered in conjunction with the difference in the rotational velocities of these regions, implies that the mass of air over an equatorial zone some 50° or 60° wide, must, upon the whole, have a relative motion from the east; and that the mass of air over all the rest of the earth must, upon the whole, have a relative motion from the west. This theoretical conclusion is corroborated by the distribution of barometric pressure. The barometer stands highest at the two parallels which, according to this theory, form the boundaries between easterly and westerly winds, while at the equator and poles it stands low. This difference may be accounted for by the excess of centrifugal force possessed by west winds, and the defect of centrifugal force in east winds. If the air simply turned with the earth, centrifugal force combined with gravity would not tend to produce accumulation of air over any particular zone, the ellipticity of the earth being precisely adapted to an equable distribution. But if a body of air or other fluid is moving with sensibly different rotational velocity from the earth, the difference in centrifugal force will give a tendency to move towards the equator, or from it, according as the differential motion is from the west or from the east. The easterly winds over the equatorial zone should therefore tend to remove air from the equator and heap it up at the limiting parallels; and the westerly winds over the remainder of the earth should tend to draw air away from the poles and heap it up at the same limiting parallels. This theoretical consequence exactly agrees with the following table of mean barometric heights in different zones given by Maury."

(The table is not material here.)

"This table shows that the barometric height falls off regularly on both sides from the two limiting zones 30° to 35° north and 20° to 25° south, the fall continuing towards both poles as far as the observations extend, and continuing inwards to a central minimum between 0° and 5° north.

"If the bottom of a cylindrical vessel of water be covered with sawdust, and the water made to rotate by stirring, the sawdust will be drawn away from the edges and heaped up in the middle, thus showing an indraught of water along the bottom towards the region of low barometer in the centre. It is probable that, from a similar cause (a central depression due to centrifugal force), there is an indraught of air along the earth's surface towards the poles, under-

neath the primary circulation which our theory supposes ; the diminution of velocity by friction against the earth rendering the lowest portion of the air obedient to this indraught, which the upper strata are enabled to resist by the centrifugal force of their more rapid motion. This, according to Professor James Thomson, is the explanation of the prevalence of south-west winds in the north temperate zone ; their southerly component being due to the barometric indraught and their westerly component to differential velocity of rotation. The indraught which also exists from the limiting parallels to the region of low barometer at the equator, coincides with the current due to difference of temperature ; and this coincidence may be a main reason of the constancy of the trade-winds."

Want of stability in these arguments will be detected by those of my readers who have followed me in applying the law of deflected centrifugal force to oceanic circulation. They need scarcely be now reminded that every particle of every fluid on the face of the earth (except at the equator and the poles) is unceasingly impelled by deflected centrifugal force in a horizontal direction from the pole to the equator ; that it is only by the resistance offered by other particles urged by a similar force, or by other constraints, that each particle is prevented from immediately obeying the impulse which urges it onwards, and that when moving horizontally in obedience to the impulse, it cannot so move without displacing an equivalent of particles and causing them to move as a stream or current backwards to occupy the space vacated. Precisely the same law of deflected centrifugal force which governs the circulation of the ocean *must* govern the circulation of the atmosphere.

Considerations arise from the difference between the nature and physical conditions of water and those of air. Water is almost inelastic, and the difference of density from compression, although exerting its influence in horizontal circulation in the ocean, was considered too small to be brought into notice before. Air, on the other hand, is extremely elastic, so much so that one half of our atmosphere

is contained in the first three or four miles above the sea-level. The effects of this elasticity of the air are peculiar. The more compressed air is the more dense, yet the greater density of the lower stratum does not prevent its rising against gravitation upon application of the slightest pressure, because the elevation and the density vary concurrently and similarly to those of the like surrounding medium. If a measure of air, for example, which at the time is heavier, be lifted into a higher level, it will have no tendency through gravitation to return; the only resistance felt in lifting it will be its own inertia and the mechanical resistance of the medium through which it is lifted.

It is easy to imagine how a lifting power might be applied in nature. Air, gases, and vapours are not equally elastic—they all differ more or less. Suppose a vapour, less expandible than air, to be taken up by it near the surface of the ground, the density of the vapour being there less than that of the air; they would together be lighter than the air on the same level, ascend, and continue to ascend, until by inferior expansibility the vaporous constituent acquired the same density as that of the air with which it was mixed. Arrived at the level in which that occurred, that level would be a *stable one of equilibrium*, and the cloud of mixed air and vapour would return to it if, by chance, forced higher or lower.

Precipitation would also, in many ways, affect density; more or less change of vapour into mist, according to situation, elevation, and surrounding conditions, would be an element in producing change of density and of the position of equilibrium.

Although a substratum of air may find a permanent position of equilibrium in respect of gravitation, it *can never do so* in respect of deflected centrifugal force under existing circumstances, and so long as it is free to move

horizontally, and herein lies the *source of constancy* in the general system of circulation under that force. However much disturbed by local conditions, and within itself, the lower portions of the atmosphere may be, they must, as a whole, by compression, have a greater density than the higher portion; they must, therefore, as a whole, have a greater centrifugal energy; they must, as a whole, obey the energy of that impulse from the pole to the equator; and they must, on the whole, have a movement corresponding in direction to that of the trade-wind, although near the ground there may be tracts of atmosphere that under local conditions have less energy, or have their energy otherwise overcome, and have to beat a retreat or make other movements. Over each parallel of latitude there must be an equal flow to and from the poles. As in the ocean, the counterflow need not be necessarily confined to any meridian. An excess of outflow in one meridian may be compensated by a deficiency in another.

By the law of hydraulics, a pressure from the side has the same effect as a similar pressure from any other direction, and as gravity offers no resistance to an immediate ascent at the equator, where opposing streams meet, a very gentle pressure from the incoming current, urged by deflected centrifugal force, would overcome the inertia and turn the stream upwards. But heat, which, for purposes of demonstration, has been left out of the question, is a valuable auxiliary in overcoming the inertia; the air expands under it, and rises, leaving the trade-winds free to flow in, subject simply to the forces by which they are urged. Equatorial heat, such as ours, is not, however, indispensable to a system of general circulation in an elastic atmosphere.

In the sphere of the sun, there is no equatorial heat similar to that of our earth; yet deflected centrifugal force must, if the solar atmosphere be elastic, prevail there, and

induce those horizontal currents, the existence of which is manifested in the belts of phenomena, notably the "spots," visible there.

Thus we see that, by the simple mechanical law of deflected centrifugal force, a grand and complete system of circulation is and must be created and upheld with certainty, regularity, and *permanency* in all revolving bodies having an atmosphere. It is not for a moment argued that heat and consequent vertical movements are not in active and effective operation; but what I do say is, that their effect is chiefly local, and that the distance intervening between the equator and the poles is too great, and the difference of weight between the hot and cold air too trifling, to admit of the over and underflow through gravity being regarded as the fundamental principle in any general scheme of circulation, even if no difficulty such as the interference of the calm belts intervened, and its adoption as such by physicists, can only be justified, as some writers have candidly confessed, by the fact that no better theory was known.

When the sun is in the solstices, why do the trade-winds pass under him without rising, notwithstanding their acquired warmth and lightness? Why do they totally disregard his influence, and travel 1000 or 1200 miles with an average speed of 20 miles an hour from him towards the equator, in direct opposition to the direction assigned them by the gravitation theory? Why do they steadily pursue their course till they reach the latitude in which the law of deflected centrifugal force bids them ascend?

There can be but one answer—between the two influences they obey the stronger, and neglect the weaker. To a certain extent the sun's influence is felt, inasmuch as the calm belt performs a vibratory movement in his direction, but the amount of it covers only ten degrees, whilst the change in his position covers forty-seven! The value of this influence,

even, is less than it appears, because the effect of the deflection of centrifugal force (and these movements arise only from deflection) is, on the equator, at its minimum, and reduced to nothing, the ascent taking place there under the lateral pressure of the inflowing currents.

It would be interesting to learn whether or not the cirri clouds occur in the region of the calms of Cancer or Capricorn. If they arise from the friction between the surfaces of the polar and equatorial currents as they pass over each other, and are visible in those calms, it would be evidence that a lower main (polar) current flowed uninterruptedly over those regions, and that the calms were only a condition of the surface of it near the water.

The same theory which requires the appearance of the cirri in the extra-tropical calms would necessitate their absence in those of the equator; and in my approaching return to Australia I shall particularly note whether or not observed facts correspond with this theory.

Deschanel does not venture to trace the effect of equatorial indraught to regions beyond the two limiting calms of Cancer and Capricorn. He, therefore, has to find other motive forces, but all his arguments seem, to me, to explain nothing but modifications of pre-existing movements; no motive force whatever is suggested by him, productive of circuits.

"That an interchange, of which there can be little doubt, of air between the frigid, temperate, and tropical regions, when considered in conjunction with the difference in the rotational velocities of these regions, implies that the mass of air over an equatorial zone some 50° or 60° wide, must, upon the whole, have a relative motion from the east; and that the mass of air over all the rest of the earth must, upon the whole, have a relative motion from the west."

I fail to see how such a consequence follows; it is inferred that "over all the rest of the earth" a west wind must, on the whole, blow; but the assumption that the west wind



must blow there is not justified, because each zone of latitude can have its independent easting and westing, quite regardless of any that may obtain in other zones. If, for the sake of argument, we admitted that every north wind must have its complement of easting, and every south wind its complement of westing, then it would follow that, in each zone, the east and west winds would be equal, because in every such zone the volume of air passing to the north must be equal to that passing to the south.

Upon such doubtful arguments Deschamel's theory of "general circulation" depends.

Deflected centrifugal force, as applied to the waters of the ocean, is applicable in its integrity to the air of the atmosphere, but in the atmosphere disturbing causes are stronger, more frequent, and more widely spread.

The calm belt at the equator is easy enough to understand; the trade-winds come in from each side, and partly by pressure from them, and partly by rarefaction through solar heat, the air in the central equatorial region quietly ascends and turns back upon itself. The force (deflected centrifugal force) operating in the circuit being a constant one, is accelerative; that is to say, a body having density will move under it with increasing velocity, according as inertia is overcome without resistance being met with. This acceleration will undoubtedly be free to take effect in the upper regions for two reasons—firstly, because the radiation and reflection of solar heat from the earth, which are such a fertile source of disturbance below, can have but little disturbing influence up there; and, secondly, because the area over which the mass moves becomes more contracted with each degree of latitude passed over, thus concentrating the effect of the momentum.

Besides one at the equator, we find in nature a calm belt on each side of it, at a distance of from 30 to 35 degrees north and south. These two calm belts, one called the

Calms of Cancer, the other the Calms of Capricorn, constitute the outer boundary respectively of the north-east and the south-east trade-winds. All the calm belts have, with the trade-winds, a vibratory motion north and south, following the declination of the sun; compared, however, with that of the sun, the vibratory movement is small, only about 10 against the sun's 47 degrees, as before stated.

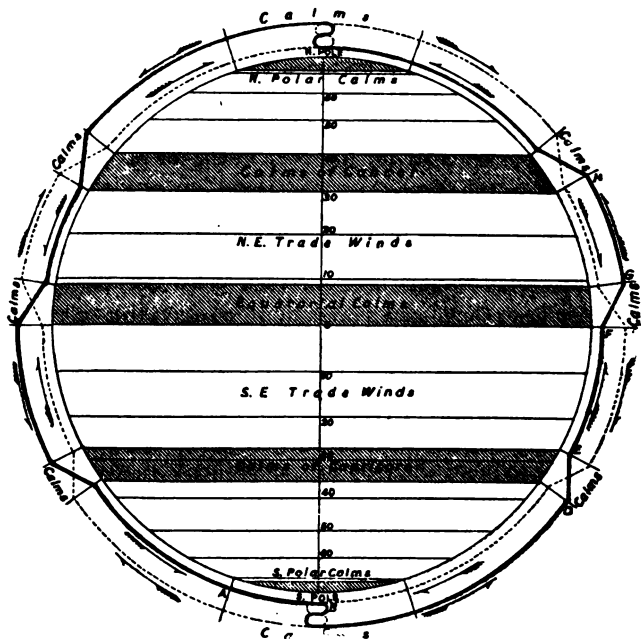


Fig. 5.

In entering the calm belt of Cancer or Capricorn we enter a region of veritable speculation as to the aerial movements. We can see nothing; we can only make inferences and draw conclusions. At this point Captain Maury (with all respect to him) lets his imagination become lively. The above

diagram and quotation give his views of the currents and of their crossings *through* each other in the *calm* regions.

In fig. 5, the position of the calms, their extent, and the presumed direction of the winds in their circuits, are taken from one of Maury's diagrams. The frequent crossings are very fanciful, and his attempt to explain them is clearly a difficult task.

"Entering the polar regions obliquely, it (the imaginary particle or tally) is pressed upon by similar particles flowing in oblique currents across every meridian; and here again (A) is a calm place or node, for, as our imaginary particle approaches the parallels near the polar calms more and more obliquely, it, with all the rest, is whirled about the pole (A to B) in a continued *circular gale*; finally, reaching the *vortex of the calm place* (B), it is carried upward to the regions above, whence it commences again its flow to the north as an upper current (C to D), as far as the calm belt of Capricorn; here it encounters its fellow from the north, they stop, descend and flow out (at E) as surface currents, the one with which the imagination is travelling, to the equatorial calm as the south-east trade-wind (E to F); here it ascends, travelling thence (G) to the calm belt of Cancer (H) as an upper current counter to the north-east trades."

In a similar way the course all round is traced by Maury, but we have followed him far enough to give an idea of it.

The possibility of the crossings at the calms has not been left unquestioned, and in answer to the question, "How are two such currents to pass each other?" Maury says:—

"In the first place, it may be said in reply, the belt of equatorial calms is often several hundred miles across, seldom less than sixty; whereas the depth of the volume of air that the trade-winds pour in it is only about three miles, for that is supposed to be about the height to which the trade-winds extend. Thus we have the air passing into these calms by an opening on the north side for the north-east trades, and another on the south for the south-east trades, having a cross section of three miles vertically to each opening. It then escapes by an opening upward, the cross section of which is sixty or one hundred, or even three hundred miles. A very slow motion upward there will carry off the air in that direction as fast as the two systems of trade-winds, with their motion of twenty miles an hour, can pour it in"

The physical difficulties are too apparent to need any lengthy demonstration.

As regards the calms of Cancer and Capricorn, a surface current with a velocity of 20 miles an hour into and an under current of 20 miles out, necessitates the *same* velocity through them, rendering a calm a simple impossibility ; and I think this physical difficulty is quite sufficient to upset the whole theory.

Independently of the physical difficulty of two currents crossing *through* each other north and south *in a calm*, the law of gravitation is subverted by making the higher and lighter equatorial warmer air change its position for one underneath the colder and denser stream from the pole. The difficulties of the position are altogether too many. What then really causes the air on the water surface to divide, as it were, and flow away on each side of the calm belt ? My attention is at once directed to the difference between the temperature of the water and that of the air, and the material question suggests itself—is the air, as it plays over the surface of the water, warmed, or is it cooled by it ? and let us follow the inquiry up. If no interchange of heat took place between the water and the air, we would expect the polar atmosphere to flow uninterruptedly, as an undercurrent, from the pole to the equator, but if the under surface of the great polar current got warmed by contact with the water, the mass so warmed would expand, have less density, lose its deflected centrifugal energy, be pressed back, and form a local, quite local, return current ; if, on the other hand, the air were cooled by contact with the water, the volume so cooled would acquire more horizontal energy in the direction in which the current was already moving, and therefore the general current would not only be not disturbed thereby, but it would be assisted. Let us next examine the actual

difference of temperature, if any, in the ocean and the atmosphere.

The temperature of the water at the surface of the ocean is certainly more uniform than that of the atmosphere. The cooling influence of water on the tropical atmosphere is well known, and equally well known is it that the surface waters by warming temper the atmosphere in the temperate and frigid zones. How does the theoretical effect of these conditions accord with the phenomena in nature? In the equatorial regions the surface water is heated by the atmosphere; consequently the atmosphere is cooled, the action of deflected centrifugal force has full play, and the normal undercurrent of the circuit continues undisturbed as the trade-wind. The cooled air flows to the equator, like the polar stream of which it is a part. The depth of this uniform current of air is estimated at three to four miles, in which space I have stated that half of the atmosphere is contained. Those fleecy clouds, the cirri, are nearly always visible, like feathers or fibrous streaks, in these regions at an altitude apparently of three or four miles; and seeing them invariably at about that same elevation, I think it may fairly be concluded that the region of the cirri is the visible boundary line between the two main currents to and from the poles and the equator. In the region of variable winds, the cirri also appear, and are the highest visible clouds; their height appears to be about the same as, or somewhat higher than, in the trade-wind region. In voyaging recently from Australia, I heard a suggestive remark made by our skipper; pointing to some fibrous or streaky cirrus clouds, he said, "Those streaks always indicate calm, fine weather;" and it quite agrees with theory. The boundary line between the two grand streams will maintain an average height, and when disturbances below occur through storms or unusual winds, the surfaces of the main currents will, by rougher

friction against each other, have a visibly puffy, wavy surface, whilst in the absence of disturbances the condensed surface will be drawn out into fine streaks, the puffy waves being reduced to tiny invisible ripples. Passing from the trade-wind region through the calm belt into the variable winds, we find there the air warmed by contact with the water, and, becoming less dense, formed into local counter-winds. Between those two regions, in one of which the air is cooled, and in the other warmed, there must exist a belt in which the temperature of the air is altered only sufficiently to retard its onward movement without creating a counter-current; the air will there remain more stationary, and the calm belt is accounted for. Turn now again to the upper current from the equator; there is nothing to interrupt its onward course, and through acceleration it must get an impetus which would carry it with great velocity over the 20th and 30th, perhaps to the 40th, degree of latitude; but here its progress will be considerably impeded by the contraction of the circles, and there can be very little doubt that, at every opportunity afforded by diminished pressure at any point below (we have seen how readily a body of air can rise or sink), a large body of air moisture-laden and warm will make a dive into the lower regions, there to discharge its moisture and to figure as warm or wet winds, and even as hurricanes, when the normal velocity under acceleration is not too much reduced in its descent.

The consideration of the origin of local disturbances and variable winds does not form part of my present subject.

My object has only been to explain *a new theory* of a constant energetic horizontal force producing a permanent circulation in the ocean and atmosphere to and from the equator, and I trust and believe that I have successfully done so.

If the water and air in the ocean and atmosphere were moved into their respective levels of equilibrium through

gravity, the work of overcoming the inertia of them might be said to be done (and so far as those movements go, the work is mainly done) at the expense of solar heat and gravity, both of which are constant sources of energy. But those vertical movements, looked upon as work done, are nothing when compared with the mighty movements horizontally which I have clearly traced to be caused by the earth's axial rotation. Hitherto we have been unable to point to any appreciable work done by that rotation, and it has been believed, in the absence of any evidence to the contrary, that the rotary motion of the earth resulted from some "primitive impulse" at one time given to it; and, for all that was known, it would have been inconsistent to start an adverse theory, because, if it were proved that the earth were urged on its axis, always to the east, it would become also necessary to prove the existence of a brake of some kind whereby to prevent that acceleration which would result, but which has not been observed.

The discovery, therefore, of the fact that the main horizontal currents are caused by the energy of the earth's rotation acquires a new and great importance, by clashing with the so-called Newtonian theory, that all movements of celestial bodies and the sea are caused solely by gravitation.

If we prove the necessity of a constant energy of eastation of the earth on her axis—as I consider I have already done—it may safely be inferred that all matter has a tendency in the same relative direction, whether that matter constitute one body or many.

If an energy of eastation be a property common to all matter, it would produce, in a cluster of two or more bodies, a parallel and opposite motion round a common centre of gravity, and act as a repulsive force. Such a parallel and opposite motion does exist in all the spheres of our solar system, *and it has never been explained!* It has always

been accepted as one of the unexplained mysteries of nature.

We need now no longer strive to demonstrate, by mathematical manipulation and erroneous deductions, that the energy of the primitive impulse of a planet suffers no loss in pushing it constantly off the line of fall towards its primary, and so preventing a descent and maintaining the stability of the orbit. We find in nature planets held permanently up against the constant pull of gravitation downwards. My proposition is, that they cannot be so held up without an opposition of forces; and if I can demonstrate that to be so, and that the loss is compensated by a constant energy of eastation, I shall have demolished a universally accepted theory of the laws of motion; a theory, the invention of which has been regarded as one of the greatest achievements of any age, supposed to be based on the soundest and most immovable foundations, and which has stood the test of examination by mathematical, physical, and mechanical philosophers for two hundred years.

What a vast new field of inquiry will be opened up? The prize is a valuable one, and it requires a bold effort to face preconceptions and secure it.

PART III.

EASTATION.

HAVING in the preceding part demonstrated that the rotary energy of the earth performs a large amount of actual work, I am—finding that no retardation takes place in consequence—quite justified in assuming that she has a constant compensating energy to the *east*, and seeing that all celestial bodies perform a movement, both axial and orbital, *to the east*, Eastation is a term that may be fairly applied to the energy in that direction. The term may also be used to signify the momentum in that direction, the momentum being the accumulated or stored-up energy of previously exerted efforts, but quite different in its operation from that of the efforts of the energy itself. The use of the term will be conveniently short in place of the general terms for that class of energy—energy round the axis, and energy in the orbit. The latter terms are general, and applicable to the movements of any bodies, impelled by any kind of force, round a centre; whereas eastation is specific, and only applicable to bodies moving freely, as fluids, in space, in the same way that gravitation is the specific term for the centripetal energy. An immense benefit will also be felt in having a distinctive term for momentum which produces centrifugal force, and for that centrifugal force or effect itself. The want of a distinction must always have led to great confusion and misapprehension. Take a line, for example, from “Cosmos,” by Baron Humboldt, who would certainly choose the most concise and correct term available.

Picard's measurement of a degree offered Newton, he says, "the means of proving how the attraction of the earth maintained the moon in its orbit, whilst *urged on its course by the centrifugal force*." Now it is quite clear that it is not centrifugal force, proper, that urges the moon in its orbit, but the impulse which, by a mechanical combination, produces the centrifugal force. Yet the term cannot be said to have been incorrectly used, because it has been customary to apply it indifferently to the force and to the effect, notwithstanding that the direction of one is at a right angle, or very nearly so, to that of the other. In coining the word "eastation," I shall, then, derive the benefit of convenience, conciseness, and, I hope, correctness.

The task I now set myself is to demonstrate that gravitation does not, *alone*, serve to account for all the motions of celestial bodies.

I am quite aware that all physical astronomers believe that it does ; but for that belief they rely, I think, solely upon an assertion of Newton ; and mathematicians seem to have confirmed the correctness of it, by the application of the infinitesimal calculus. That application, however, may be a *misapplication* ; and I think it is so because it is restricted to each moment of time taken independently of all other moments, and the forces concerned are taken as they are found "sensibly" to exist at that moment, and without reference to the state of them before passing into, and after passing out of, it.

I have carefully read Newton's "Mathematical Principles," and I find absolutely nothing in them to demonstrate, or even to indicate that he thought of demonstrating, whether the impulse—the eastation (I may as well use the term at once) suffered no loss in its combinations with gravitation, or whether it suffered a loss with compensation from eastating energy. There is nothing to indicate that he

made that enquiry ; if he made it, I can only conclude that the result was not sufficiently satisfactory for him to make it public. But really when we remember that Newton was the first to open that page in the book of Nature, and that his health was declining when he completed his theory of gravitation, it is not surprising that he should have left untouched, perhaps even unthought of, the theory of eastation. He found the *momentum* of eastation in the main, or on the whole, invariable, and for the purpose of combination with gravitation he applied the force just as he found it.

In works on Natural Philosophy we rarely find any attempt, and then only bungling ones, to demonstrate by the known laws of motion the stability of planetary orbits. More recent writers make no attempt at it ; they content themselves with saying "Newton proved" it. They demonstrate various other effects which admit of demonstration, but when it comes to the planetary orbit, it is disposed of by "Newton proved," &c., a common confession of weakness which encourages me to proceed.

Newton certainly does use words which may be, and have been, taken to mean more than he may have intended to say. After finishing his work, he, in his "General Scholium," or retrospective comment, uses the words following :—

"Hitherto we have explained the phenomena of the heavens and of our sea, by the power of gravity, but have not yet assigned the cause of this power. This is certain that it must proceed from a cause that penetrates to the very centres of the sun and planets, without suffering the least diminution of its force ; that operates not according to the quantity of the surfaces of the particles upon which it acts (as mechanical causes use to do), but according to the quantity of the solid matter which they contain, and propagates its virtue on all sides, to immense distances, decreasing always in the duplicate proportion of the distances. Gravitation towards the sun is made up out of the gravitation towards the several particles of which the body of the sun is composed ; and in receding from the sun, decreases accurately in the duplicate proportion of the dis-

tances, as far as the orb of Saturn, as evidently appears from the quiescence of the aphelions of the planets ; nay, and even to the remotest aphelions of the comets, if those aphelions are also quiescent. But hitherto I have not been able to discover the cause of those properties of gravity from phenomena, and I frame no hypotheses. For whatever is not deduced from the phenomena, is to be called an hypothesis ; and hypotheses, whether metaphysical or physical, whether of occult qualities or mechanical, have no place in experimental philosophy. In this philosophy particular propositions are inferred from the phenomena, and afterwards rendered general by induction. Thus it was that the impenetrability, the mobility, and the impulsive force of bodies, and the laws of motion and gravitation were discovered. And to us it is enough that gravity does really exist, and acts according to the laws which we have laid down, and abundantly serves to account for all the motions of the celestial bodies, and of our sea."

It must, as I said, be remembered that at the time those lines were penned by Newton, the enquiry into the nature of forces by which the planets are urged, was quite in its infancy, and he was led into it, in a measure, by accident. His senior contemporary, Galileo, discovered the law of acceleration by which all bodies near the earth fall to it in, as he thought, parallel lines. Newton conceived the idea that they all fell to one *central* point in the earth (consequently not in parallel lines), and by an attraction which resided in all bodies, and that therefore all bodies in space must be constantly attracting each other. If so, a body—the moon, for example—free to move in space, would be drawn to the earth, and would either fall to it, or be prevented from falling by some opposing force. Newton conceived that, in a small arc of the moon's orbit, the direction of gravitation was all but parallel, and that therefore gravitation must have effect. He noticed that the moon was being constantly pulled from the rectilinear path, which, but for gravitation, she would have pursued ; and he set himself about ascertaining whether or not the movement performed

in being drawn from that path corresponded exactly with the movement which she would perform in falling to the earth according to his theory of gravitation. If he could demonstrate that they did correspond, the important deduction might be made that gravitation to the earth acted in the moon, and kept her in her orbit, by the same law by which it acted on and urged bodies near the earth's surface, deflecting her from the straight path which, otherwise, she would have pursued. Now the meaning of the expression of the moon being "kept in her orbit" is what is open to doubt, or what may have been misunderstood. Newton's whole work on the motion of bodies in space, is a mathematical analysis of the effect of gravitation in producing curves of different kinds, and showing that gravitation was the only force which caused the body to swerve from the path of a right line which would be natural to it when once in motion. All observed motions of bodies moving in space must be and are curvilinear, because every centre of gravity is a point, and every known particle of matter must be under an influence, however small, of other particles. Seeing then that celestial bodies have no other than a curved motion, and that he had been studying no other motion, Newton cannot be taxed with a want of preciseness in omitting to express a qualification which was so self-evident. Taken with the context, and with the mathematical portion of the work, we may take the words "*to account for all the motions of the celestial bodies,*" as meaning "to account for all the curves and orbits" of those bodies. In the whole of the mathematical work there is not the faintest allusion whatever to the way in which that force operates which urges a planetary body about its primary, and produces, by a mechanical arrangement, that angular or centrifugal effect through which gravitation is capable of being equilibrated; that force he always took as he found it to exist.

Newton demonstrated that *one* of the motions (the tides) of the sea was caused by gravitation, and in the paragraph quoted he says that *all* motions of our sea are caused by gravitation. Would any one pretend to say that his meaning can be accepted literally in this case? If not, why are we to be bound to accept literally the words relative to the motions of the planets written in the same line? If he commit a mistake in the first and more simple case of the sea, it is possible for him to be mistaken in the other one.

Before I enter upon a consideration of this, however, it will be better that I show some incongruities of the present theory.

The difficulty of understanding any explanation of the combination of the forces has led to various conceptions about the stability of the solar system. A very general idea is, that it is maintained by the ellipticity of planetary orbits whereby the planet, in its passages from aphelion to perihelion, receives, *through gravitation*, sufficient acceleration in its fall to send it up again to aphelion. The theory is a plausible one, but against it we have the overwhelming fact that some of the Satellites have circular orbits, and what is law for one planet must be law for the other; those Satellites maintain their velocity without the assistance of the pendulum-like motion peculiar to ellipticity, therefore ellipticity is not a prime condition of the planetary orbit, and cannot be the means of supplying an energy opposed to gravitation.

Deschamel, in his "Natural Philosophy," seems to adopt some idea of the kind, when he says :—

"Whenever kinetic energy (energy of motion) is increased at the expense of potential energy (energy of position), the forces concerned do an amount of positive work equal to the amount by which the former is increased or the latter diminished. On the other hand, whenever potential energy is increased at the expense of kinetic

energy, the forces do negative work equal in absolute value to the energy thus transferred. Instances of the former kind of transfer are furnished by the motion of a falling body, *and the motion of a planet from aphelion to perihelion*; instances of the latter kind are furnished by the motion of a body thrown upwards, *and the motion of a planet from perihelion to aphelion.*"

A planet certainly does fall from aphelion to perihelion, but, as regards the falling motion, it ceases for a moment at perihelion as well as at aphelion, and the position of greatest energy of this motion is at a mean distance from the primary, past which point, above and below, the planet rises and falls as if performing the movement under an accelerative, attractive force, whose central point is at that mean distance. That mean distance is the distance at which gravitation and centrifugal force are in equilibrium. Outside of it, gravitation becomes the superior energetic force; inside of it, the inferior. The two forces oppose each other; their effect may be compounded into one resultant, which is equal to the difference between them, and it becomes practically a single force directed to the mean line of equilibrium; and, regarded in this manner, the planet has its greatest potential energy in the mean position, and the greatest kinetic energy at perihelion and aphelion. The greater *orbital* velocity of the planet at perihelion is a condition productive of the greater and superior centrifugal effect in that position, but I question whether it could be derived *exclusively* from the acceleration through gravity.

The question, however, has been *considered* to be removed beyond the region of doubt by the application of the calculation of infinitesimals, proving that the primitive impulse suffers, in opposing gravitation, a loss of energy so infinitesimally small in each moment of time, as to be the nearest possible approach admitting of expression to an absolute nothing.

In enumerating the important results of the invention of

the infinitesimal calculus, Humboldt omits (whether intentionally or not cannot be known) this, perhaps the most important one of all. He says :—

“The invention of the infinitesimal calculus characterises the close of the (seventeenth) century ; and, strengthened by its aid, human understanding has been enabled, during the succeeding century and-a-half, successfully to venture on the solution of the problems presented by the perturbations of the heavenly bodies, by the polarisation and interference of the waves of light, by the radiation of heat, by electro-magnetic re-entering currents, by vibrating chords and surfaces, by the capillary attraction of narrow tubes, and by many other natural phenomena.”

Surely the stability of the orbit itself should have been mentioned before the perturbations in it ? The following important proposition is put and answered by an astronomer who made this one of his special studies, and who may be taken as an exponent of the popular idea. The problem which presents itself is this :—

“If the central force lodged in the sun has the power to cause a planet to diverge from the straight line in which, but for this, it would have moved—if it draw it into a curved path, will not this central force, which is ever active, finally overcome, entirely, the impulsive force originally given to the planet, draw it closer and closer to the sun in each successive revolution, in a spiral orbit, until finally the planet shall fall into the sun, and be destroyed for ever ? This question arises independent of the extraneous influence which the planets exert over each other. It refers to a solitary globe revolving round the sun, under the influence of a central force which varies its action as does the law of gravitation. The problem has been submitted to the most rigorous mathematical examination, and a result has been obtained which settles the question in the most absolute manner. The amount by which the central force, in a moment of time, overcomes the effect produced by the primitive impulse, is a quantity *infinitely small*, and of the *second order*. If it were found to be *infinitely small* in each moment of time, then might it accumulate so that, at the end of a vast period it might become finite and appreciable. But because it is of the *second order of infinitely small quantities*, before it can become an

infinitely small quantity of the *first* order, a period equal to infinite ages must roll by, and to make a finite appreciable quantity out of this an infinite cycle of years must roll round an infinite number of times! Such is the answer given by analysis to this wonderful question. 'Is there no change?' demands the astronomer. 'Yes,' answers the all-seeing analysis. 'When will it become appreciable?' asks the astronomer. 'At the end of a period infinitely long, repeated an infinite number of times,' is the reply." ("Planetary and Stellar Worlds," by Professor Mitchell, of the Cincinnati Observatory, U.S.)

The feature of these arguments is that they are applied to the smallest possible moment of time, and the smallest possible space travelled over, in order to distort the natural facts, and make the directions of the centrifugal force, which are never in fact parallel, appear *sensibly parallel*. The error made here, and which will be again referred to, is the all important one that the effect of the "amount by which the primitive impulse is overcome" is taken to accumulate as the time instead of as the square of the time.

Deschamel follows in the same groove. In explaining the calculation of the amount of centrifugal force in an arc from a point *M* to another point *M'*, the calculation is given as an exact one, but it is naively confessed that it is correct, "provided that the time considered and the distance moved in that time are so small that the directions of the centripetal forces at *M* and *M'* are sensibly parallel." The assumption of sensible parallelism seems, however, to have the authority of Newton, who demonstrates his theory of gravitation by showing that the moon is, in a sense, always falling to the earth in accordance with it; he says:—

"The mean distance of the centre of the moon from the centre of earth will be (in the moon's octants) nearly $60\frac{2}{3}$ of the greatest semi-diameter of the earth. For the greatest semi-diameter of the earth was 19'658'600 Paris feet, and the mean distance of the centres of the earth and moon, consisting of $60\frac{2}{3}$ such semi-diameters, is equal to 1'187'379'440 feet. And this distance is to the distance of

the moon's centre from the common centre of gravity of the earth and moon, as 40'788 to 39'788 ; which latter distance therefore is 1,158,268,534 feet. And since the moon, in respect of the fixed stars, performs its revolution in 27 d. 7 h. 43 $\frac{1}{2}$ m., the versed sine of that angle which the moon in a minute of time describes is 12,752,341 to the radius 1000'000000 000000, and as the radius is to this versed sine, so are 1'158'268'534 feet to 14'7706353 feet. The moon, therefore, falling towards the earth, by that force which retains it in its orbit, would in one minute of time describe 14'7706353 feet. And if we augment this force in the proportion of 178 $\frac{2}{3}$ to 177 $\frac{2}{3}$ * we shall have the total force of gravity at the orbit of the moon. And the moon falling by this force, in one minute of time would describe 14'8538067 feet. And at the 60th part of the distance of the moon from the earth's centre, that is at a distance of 197,896,573 feet from the centre of the earth, a body falling by its weight would, in one second of time, likewise describe 44'8538067 feet. And therefore at the distance of 19,615,800, which compose one mean semi-diameter of the earth, a heavy body would describe in falling 15'11175, or 15 feet 1 inch and 4 $\frac{1}{4}$ lines in the same time. This will be the descent of bodies in the latitude of 45°. And by the foregoing table, to be found under prop. 20, the descent in the latitude of Paris will be a little greater by an excess of about $\frac{1}{3}$ part of a line. Therefore by this computation heavy bodies in the latitude of Paris falling in vacuo will describe 15 Paris feet 1 inch 4 $\frac{2}{3}$ lines very nearly in one second of time. And if the gravity be diminished by taking away a quantity equal to the centrifugal force arising in that latitude from the earth's diurnal motion, heavy bodies falling there will describe in one second of time 15 feet 1 inch and 1 $\frac{1}{2}$ lines. And with this velocity heavy bodies do really fall in the latitude of Paris, as we have shown in prop. 4 and 19."

It is all very well for Newton for the particular purpose he had in hand, to disregard the convergence of gravitating directions. For the purpose of his computation, the result might be not sensibly affected. It would have been a very different thing if he had been measuring with mathematical accuracy the amount of resistance felt by the impulse in producing centrifugal effect as others have since done. As it is, the computation of gravitation is by no means an exact

* This difference arises from the effect of the sun's attraction.

one, nor can I believe it was intended to be so, for there are two sources of error in it, which, in an exact calculation, could not possibly be disregarded by a Newton.

The errors are these :

1st. Through the arc described by the moon in one minute of time the direction of gravitation is assumed to be always parallel, his "versed sine" being one side of an arbitrarily constructed rectangle.

2nd. The rate of acceleration is less in the arc than it would be if the moon were deprived of the motion of eastation and let drop direct to the earth.

This will be better explained by a diagram.

E is the earth ; M the moon ; $M c$ the arc described by the

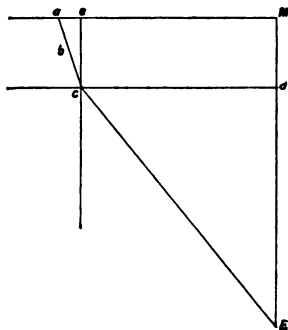


Fig. 6.

moon in one minute of time ; $M a$ the tangential distance which, but for gravitation, would have been performed in the same time, and with the same velocity ; $a b c$ is the path through which the moon performs the falling motion during the minute, and which is curved in consequence of the relative position of the earth moving constantly to the side, and thus constantly altering the direction of gravitation. In the rectangle the two sides $M d$ and ec are equal, so it will be

seen at a glance that the versed sine Md cannot be equal to the chord ac , much less to the arc abc , and the difference is still further increased by the differential rate of acceleration at the points d and c . Therefore, accurately, Newton's versed sine Md neither represents the space through which the moon would fall by gravitation if deprived of its eastation, nor the space through which it is in nature actually drawn from the tangential line by gravitation; hence his argument must not be used for purposes of exact mathematical calculation.

The modern demonstration of the motions of the planet is not found in Newton's works, nor any thing approaching to it in character. As an example, the following may be taken from a standard work—Comstock's "Natural Philosophy."

"*Motions of planets.*—We have seen in a former part of this work that all undisturbed motion is straight forward, and that a body projected into open space would continue perpetually to move in a right line, unless retarded or drawn out of this course by some external cause. To account for the motion of the planets in their orbits, we will suppose that the earth at the time of its creation was thrown by the Creator into open space, the sun having been before created and fixed in his present place. Under compound motion it has been shown that when a body is acted on by two forces perpendicular to each other, its motion will be in a diagonal line between the direction of the two forces.

But we will again here suppose that a ball be moving in the line in x , fig. 224, with a given force, and that another force half as great should strike it in the direction of N , the ball would then describe the diagonal of a parallelogram, whose length would be just equal to twice its breadth, and the line of the ball would be straight, because it would obey

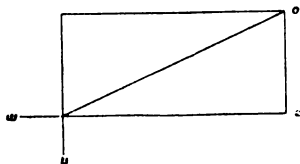


Fig. 224.

the impulse and direction of these two forces only.

"Now let A , fig. 225, represent the earth, and S the sun; and supposing the earth to be moving forward, in the line from A to B , and to have arrived at A , with a velocity in a given time, and without disturbance, to carry it to B . But at the point A the sun S acts

How deductions so palpably opposed to facts can be made, is surprising to me.

It is stated that the diagonal curve AD is performed in the same time as the line AB ; but how can that possibly be, when in nature the velocity continues unaltered? The proposition is too palpably erroneous.

Mitchell puts forward similar views :—

“If a body received an impulse, which singly would cause it to move 30 feet in a second, on a line of the direction of the impulse, and at the same instant another impulse be communicated in a different direction from the first, and which, if acting alone, would cause the body to move on a line of direction of the second impulse 40 feet in one second; under the joint action of these two impulses the body will move in a direction easily determined from those of the impulsive force, and will fly with a velocity of 50 feet in the first second of time. Such is the prevalence of this beautiful proposition, that no falling, flying or moving body, whether it be the rifle ball, the cannon shot, or the *circling planet*, is free from the imperious sway. Strike the knowledge of this great truth from existence, and the magnificent structure which modern science has reared falls in ruins at a single blow.”

The motion of the circling planet is not in accordance with this law. With an initial motion of 30 feet a second, a planet, in its orbit, notwithstanding the additional motion from gravitation, describes a diagonal of only the same distance (less if the chord be taken) instead of the increased distance, which, as Mitchell correctly says, is imperatively required by the laws of motion when there is no opposition.

The way in which such inconsistencies are smoothed over by scientists is really singular. This is how Deschamel accommodates himself to observed facts :—

“When a body moves so that all its points are at every instant travelling in the same direction (that is in parallel directions and towards the same parts), and with equal velocities, it is said to have a motion of translation. All straight lines in a body so moving remain always parallel to their original positions, and conversely; hence this property may be taken as the definition of movement of translation. Every possible motion of a *rigid body* can be resolved

into motion of translation *accompanied by motion of rotation*, and the resolution can always be so effected that the axis of rotation at any instant shall be parallel to the direction of the movement of translation. It is always possible, and is generally convenient, to regard the motion of a *rigid* body under the action of any forces as compounded of a motion of translation *of the body as a whole*, and a rotation of the body about an axis passing through its centre of mass.

"When two or more bodies, or parts of the same body, are free to move, it is impossible for any action exerted between them to alter the common centre of mass. It is also impossible for such action to alter the total angular momentum about the centre of mass. For example, when an animal is either jumping or falling, no movement that it can make in mid-air without touching other bodies can either alter the motion of its centre of gravity, or cause part of its body to rotate in one direction without causing the remainder to rotate in the opposite direction.

"This principle is of great importance in the movement of the heavenly bodies. For example, neglecting any general movement which the solar system as a whole may have in space, we are entitled to assert that in whatever direction the common centre of gravity of the planets may be moving at any time, the centre of gravity of the sun must be moving in a *parallel and opposite direction*, inasmuch as the centre of gravity of the whole system, consisting of sun and planets, remains always at rest."

Now why, in regard to the movements of the Solar System is Deschamel so uncertain of his ground that he has to use words so suggestive of doubt, as "we are entitled to assert." The very expression challenges us to look searchingly through what had been said for just grounds for the assertion; and after searching carefully (be it remembered that the sun, and the planets, are, to each other, as fluids) I fail to find any at all, unless we consider a questionable use of the word "or" in the first line of the second paragraph to be one. The subject is the movement of a body, or parts of the same body, and the introduction of the word "or" before the words "parts of the same body," is utterly mysterious. How absurd the signification if we omit (as we are entitled to do) the words "or parts of the same body;" thus :—"When two or

more bodies are free to move, it is impossible for any action exerted between them to alter the motion of their common centre of mass." Then read the line thus: "When two or more bodies, being parts of the same body, are free to move," or even thus: "When two or more bodies, parts of the same body, &c.," how very different and perfect the sense becomes. The introduction of the word "or" gives a colourable excuse for the reference to justification. I can only imagine oversights of this kind to occur through an intense desire to uphold a defective theory. Whether the mistake originated with Deschamel or with his translator, Professor Everett, I am unable to say, as I have read only the translation.

In nature the opposite and parallel motion does really exist. The fact must excite our surprise and curiosity; and in it lies my heaviest bill of indictment against the sufficiency of gravitation as a sole force. Gravitation cannot by any mathematical manipulation be shown to produce the opposite and parallel movement. And our only reason for saying that that motion can occur, is, that we see it exist in nature.

If a stick be thrown promiscuously into the air it will receive two motions—one of translation and one (through rigidity of the parts of the body) of motion about the axis. If it be, first, weighted lightly at one end and heavily at the other, the centre of motion will be at the centre of gravity, and, of course, nearer the heavier end.

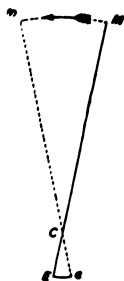


Fig. 7.

If a small body, M, Fig. 7, be attached by a rod to a larger body, E, and thrown freely into the air, distance of the parts and mass have to be taken into account in ascertaining the centre of motion. When this is found, the bodies exert upon each other, through

rigidity of the parts, an angular energy which keeps the

relative motions of the bodies *uniform*, as well as opposite and parallel.

By *leverage* of the arm, a uniformity in the motion is secured. Sever the connection of that arm, and where can we look for the power to supply the necessary angular force ? In nature we see the fact of the uniform motion before us, each body being at the same time free to move, so it is only reasonable to conclude that each body is possessed of its own energy in upholding its relative position.

In nature the moon at M, and the earth at E, move round the centre of gravity, C, each with exactly that velocity to make it correspond with the motion of the other. The moon is supposed to have been placed in her orbit by the hands of the Creator, with an initial velocity ; which velocity is said to remain unimpaired and unimpoverished in all time. Seeing that the earth describes her small orbit round the centre of gravity in exactly the same period and concurrently with the moon, it follows that the earth must have received at the same time a primitive impulse of exactly the same value. What a very curious position we have to contemplate, when we consider that the primitive impulse could only be given by one simple force, and as one simple motion, yet suited exactly to all future requirements. Some writer on the subject—I forget who—when writing about this, stated that the movements of the Solar System were so delicately adjusted that the entrance of another planet into it would upset the equilibrium, and lead to universal confusion. Humbly, indeed, he ventured to suggest that the various planets might have been placed so as to be more free from difficult complication, and he even ventured to sketch out his improved scheme, which he wondered the Creator had not adopted in preference to the present one ! Difficulties do not stop here. The moon during six months of the year describes a smaller orbit and with greater

velocity than during the other six months; so that we should imagine that the earth would be left behind in her small orbit. Nothing of the kind occurs; she bounds forward when the moon bounds, she lags when the moon lags; harmony in the angular movements is preserved as perfectly as if the two bodies were connected by a self-adjusting rigid rod. Let us at once banish all doubt, and admit that there must be some energetic force in nature which exerts a compensating and balancing power, able to meet all emergencies, so that if even a new sphere a thousand times larger than the sun were thrown into our vicinity, it would have to approach us with a motion from west to east, and, by existing forces, immediately produce an opposite and parallel motion in every body brought within its influence, and acquire, simultaneously with its approach to the other spheres, a relative power of gravitating attraction, and a relative power of eastation productive of a self-adjusting repulsive effect. Under such a simple supposition all the spheres would seek their own permanent orbits, approaching yet avoiding each other, with a motion manifested in the constituent parts of the tails of the comets which occasionally adorn our sky—a motion that has been ascribed by theorists to a power of repulsion supposed to be possessed by the sun, but a power of such startling intensity as to be incredible, and incompatible with our present knowledge of natural forces.

Having hazarded the assertion that there is no known force explaining the parallel and opposite motion of a primary to its planet, let us for a moment follow the moon, and see what should be the effect of gravitation upon the earth. In its parallel and opposite motion to the moon, the earth performs in one month a small orbit round the common centre of gravity. The earth and the moon concurrently each performs its own orbit. The question arises

—If the projectile motion of the moon were derived, as is supposed, from a primitive impulse, how is it that the earth in its independent small orbit round the common centre, has had given to it, also, a primitive impulse with exactly that intensity which ensures permanence and absolute agreement and concurrency of their periodic times? The answer is fraught with difficulties. In like manner the sun performs relatively to each planet independently, a small orbit in a parallel and opposite direction. It is true that all those orbits are compounded into one in which the sun moves, as Deschamel has told us, in a direction parallel and opposite to that of the common centre of gravity of all the planets. If the question was difficult relatively to one planet, how much more difficult does it become when the planets are multi-

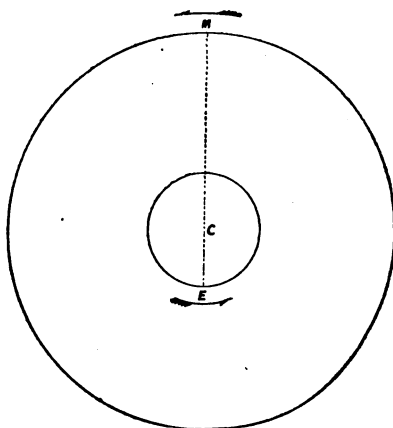


Fig. 8.

plied? The sun must have received exactly such a primitive impulse as would meet all future planetary motions. Then the question arises, Does the sun possess an opposite and

parallel movement to each of the major planets only, or to all planets, satellites, and asteroids in their vast numbers?

If gravitation alone cannot produce an opposite and parallel motion, what movement should we expect to find it produce? How should we expect the earth to move in obedience to the forces impressed upon it by the moon's circling round her?

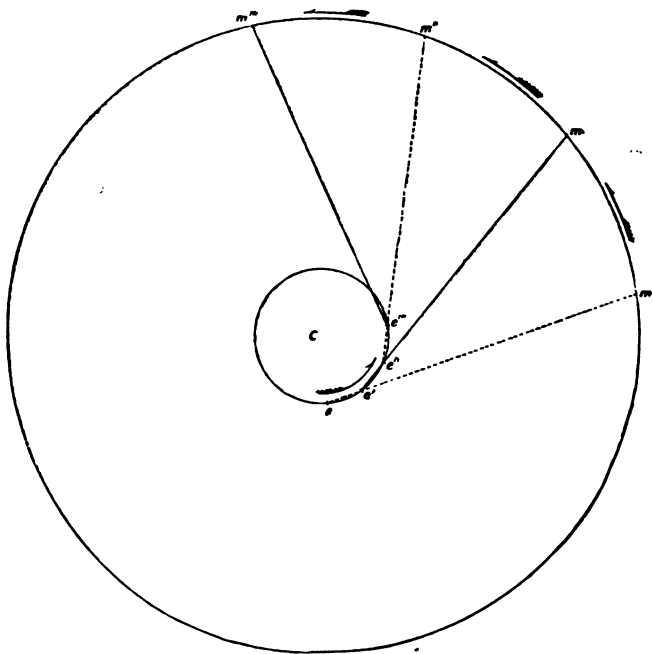


Fig. 9.

In Fig. 8 the relative positions of the earth, E, the moon, M, and the centre of gravity, C, are as found at any moment in nature. Both the earth and the moon find themselves in

a state of equilibrium between the two forces of gravitation and centrifugal force, with the line connecting them passing through the centre of gravity.

Suppose the moon to receive an accession of velocity as she sometimes does, what force is there to communicate to the earth, angularly, a similar acceleration? I can see none. Both bodies move independently of each other, the equilibrium is an unstable one, and if the moon do not fall to the earth, the earth must fall to the moon, unless prevented by her inertia or a permanent energy of her own sufficient to keep them asunder as if by an act of repulsion.

If the earth have no constant eastation of her own, she might be expected to settle down into a position and condition of being pulled round in her small orbit by the moon, as shown in Fig. 9; but then the angle of opposition between the forces acting on the moon would be too palpably a drag on her momentum; and, besides, the position is one which is not found in nature, and need not be further discussed.

Fig. 10 shows the relative positions of the moon and earth whilst moving in their respective orbits.

The moon passes the point m towards m' , at the same time that the earth passes the point e towards e' . One arrives at m' when the other arrives at e' , the motion being in each instant of time opposite and parallel; and similarly, the movements are made to the points m'' and m''' and to e'' and e''' . This motion is as we find it in nature.

Now even granting, for the sake of argument, that the earth had received a primitive impulse which impelled it in the orbit $e e' e'' e'''$, what mechanical arrangement can possibly regulate the angular momentum of the earth so as to make her period of revolution correspond with the constantly varying period of the moon, shortening together when the earth is in perihelion, lengthening together when in aphelion?

Returning still to the moon, it is convenient to regard her

orbit as circular. The circular orbit is a quite natural one, for Jupiter's first two satellites have so little ellipticity in their orbits that it is inappreciable, whilst the ellipticity of the other two is only 0.0013 and 0.0072 respectively.

In a moon's circular orbit, she would have, at any individual moment of time, a momentum perpendicular to the

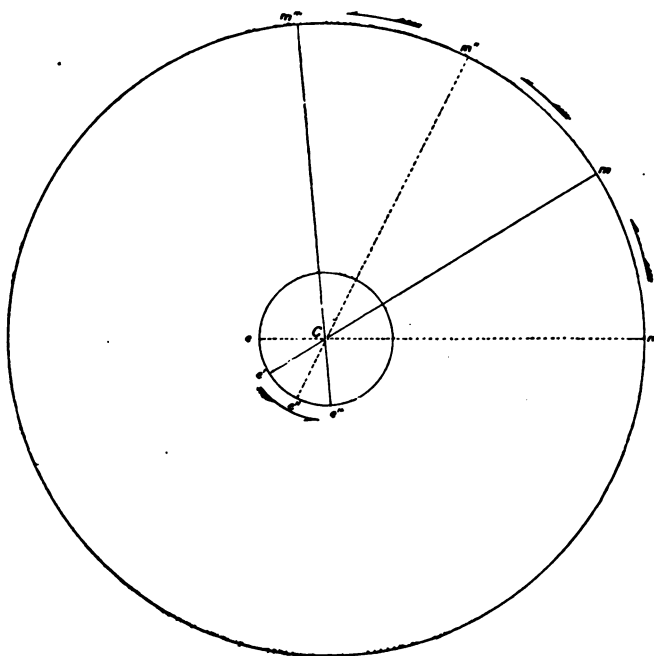


Fig. 10.

direction of gravitation. Hence it is said that the centrifugal effect in each moment is inappreciable; but theorists take that moment and every succeeding moment independently of each other, and come to the same conclusion for

each. I do not think they can justly be separated. I must take them together, and as I find that velocity and momentum should increase in passing from one moment to another, I cannot neglect such a difference between theory and nature.

We will assume that there is no opposition of forces in the first moment ; if there be no opposition in the second moment the result must be an increased velocity. In nature we find no increased velocity under the combined influences of the forces, therefore there must be opposition. This is a conclusion that I think no amount of mathematical manipulation will enable us to avoid coming to ; and the same argument will apply to the third and fourth, and to every succeeding moment of time.

I cannot but look upon it as an error to assume that in each, or any, moment of time in a circular orbit, a planet can be struck by the force of gravitation *perpendicularly* to the direction of its movement at the time, because the very existence of centrifugal force depends upon that of opposition at an angle. Without such opposition there can be no centrifugal effect, and upon the measure of the angle depends, directly, the intensity of the effect, or energy from the central point.

For this reason the propriety of the application of the infinitesimal calculus is at once made questionable. I do not say that the calculation of infinitesimals is an erroneous one, but I maintain that the effect, which it must be observed it really does admit, would increase in its accumulation, not *as the time*, as mathematicians have assumed, but *as the square of the time*.

The calculation might with equal propriety be applied to the effect of gravitation in each infinitesimally small moment of time taken separately, and this reminds me of an anecdote I once heard told. A person wagered that he could prove

theoretically the impossibility of one point in space reaching and passing another, and he did so by demonstrating that, in the reduction of the distance between them, there must always yet remain a distance divisible again and again.

All these cases I look upon as catches, with this difference, that in the wager case time is overlooked, whilst in the question of centrifugal effect the *rate* of accumulation is neglected.

Let 1, 2, 3, 4, (Fig. 11) represent equal intervals of time in a moon's circular orbit. It is evident that the direction of motion between no two intervals can be parallel to each other.

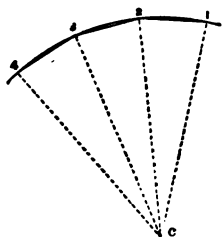


Fig. 11.

The angles 1, 2, c, and c, 2, 3, must be acute angles, and they must be similar and equal. These are the only possible angles compatible with a circular orbit; if the angles differed a spiral orbit would result. The nature of centripetal action will perhaps be

better understood, if we consider the force to be a push by a constraining guide, instead of an attractive pull. A marble rolling round a circular dish may serve as an illustration.

In Fig. 12 the marble strikes the side of the dish at each of the points 1, 2, 3, 4, and rebounds from it to the next; each time it strikes the dish, action and re-action are produced. The resistance of the constraining rim of the dish is similar in its effect to the constraining force of gravitation, and we have to consider whether the marble can leave the point 2, after striking it, with the same velocity with which it approached it.



Fig. 12.

It must be admitted that the angle of incidence is less

than a right angle, therefore the two forces must strike each other in opposition, and the law of the parallelogram of forces proves that, in that case, an amount of negative work is done, the amount of that negative work depending upon the angle of opposition, and increasing, until, on the forces meeting in parallel directions, the whole of, at least, one force is expended unproductively of motion, as likewise an equal amount of the other force, only the balance of the latter remaining available. In the case of the dish there is no energy but that of the momentum, and any loss of energy must be subtracted from it. When gravitation is the constraining guide the same arguments apply, only there is a double loss, one to the momentum and one to gravitation. I have little doubt that the idea will at once arise that the energy of gravitation compensates for the loss of the momentum.

A body thus moved by a constraining guide from its parallel direction, must, in virtue of its momentum, press at an angle against it, and, according to that angle, the pressure will be more or less: that pressure is centrifugal force, and the amount of it determines the amount of energy, which, in the production of it, has to be deducted from the momentum. The meeting in opposition is the cause—the pressure or centrifugal force is the effect. Increase the cause, and you increase the effect: destroy the cause, and there is no effect. They go inseparably together.

Now the all-important question arises, can gravitation compensate the loss to momentum in addition to its own loss? I say, distinctly, no—it cannot do double work—it cannot do more than equilibrate its own fall, and it does that in maintaining its mean distance from its primary, leaving the loss of momentum to be compensated from some other source. The energy of gravitation of one moment cannot do more than equilibrate that of the next.

I do not mean to say that a circular orbit cannot be pro-

duced by two forces without loss—I will, indeed, produce one theoretically—but then it will be observed that there is, in it, no opposition of forces, therefore no centrifugal effect.

On the gravitation theory there are three forces necessary to the production of a planetary orbit.

Firstly. Gravitation.

Secondly. An explosive or similar force, sufficient to have projected it from the primary, and giving it its energy of position ; and many people hold that the planets are chips from the sun, projected from it by explosion.

Thirdly. The angular momentum by which it moves round the central point.

For the purpose of my illustration I will now take only the first and second forces.

A body (without the angular momentum) is let fall from a point towards a distant imaginary point of attraction. During the fall the whole of the energy of attraction is transferred to the body, and is vested in it as energy of motion, or momentum. This momentum is sufficient to carry it past the point of attraction, and to project it to a distance equal to that from which it started ; thence it will fall again, and according to the known laws of motion, the vibratory movement would become perpetual, and this appears to me to be the simplest form in which gravitation can be made to equilibrate itself with a perpetual motion.

Then imagine a line, of any length, to have, at every point, an equal power of attraction. A body let fall towards it, would vibrate past it backwards and forwards ; even if a force, accelerative or not, but always at a right angle to the direction of attraction, were applied to it, the distance performed in a parallel direction would remain unaltered, and the vibration unimpaired.

Now, perpendicularly to the first line A B (Fig. 13),

imagine a second line, C D, similarly attractive, and the moment the body commences its fall from A, let it be struck perpendicularly to it, by the force which would be required to send it in a parallel direction from E to D, all the points, A, D, B, C, being equidistant from the central point, E. It is easy to prove that the resultant path will be a perfect circle cutting the points, D, B, and C. In an orbit performed under such circumstances,

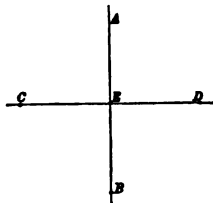


Fig. 13.

the two forces never meet in opposition, therefore there is no work done beyond each force of to-day counterbalancing the force (transformed into momentum) of yesterday. For the performance of work of any other kind an additional constant force is necessary.

Energy produces velocity, and that velocity is as the energy. Velocity round a centre of attraction produces centrifugal force, but with this peculiar feature—that the centrifugal force is as the square of the velocity; hence, in approaching the primary, the centrifugal force of a planetary or cometary body increases with amazing rapidity—greatly faster than inversely as the cube of the distance, consequently much faster than any waste that can occur from negative work; each planet, then, will float in equilibrium at a distance from its primary determined by its own power of eastation; hence the distances of all the planets and their orbital periods are governed by one law, and we here find the key to the grand law, that the squares of the times of revolution of two planets are as the cubes of their mean distances—a law first observed by Kepler, and promulgated by him as his third law of planetary motions. His first and second laws—namely, 1st. Each orbit of a planetary body is an ellipse, in one of whose foci the sun is situated; 2nd.

Each planetary body describes in equal times equal sectors round the sun—will also doubtless now soon find their explanation.

The earth, in virtue of its own power of eastation, holds its present mean position of equilibrium; but if at any moment it acquired a greater power, its line of equilibrium would immediately be removed to a greater distance; and so with all the other planets.

I cannot suggest anything more to make my case complete, so far as regards the imperativeness of loss of momentum, following the production of centrifugal force, and its restoration in the maintenance of a circular orbit, and now we may regard some other results that would follow the existence of the force of eastation as one of the natural forces.

Before doing so, however, I will point out what I conceive may be the force which confines the orbits of all the planets within one plane, and my demonstration is chiefly based upon experiment.

On a sheet of water I float a magnetised bar of steel, B

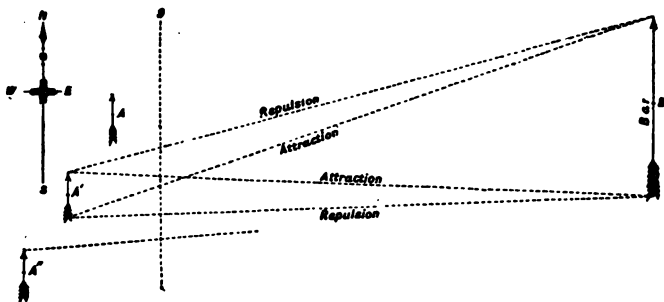


Fig. 14.

(Fig. 14), and perpendicular to it, in a line from its centre, a magnetised needle at A; in such a position that they lie east and west of each other, and at such a distance that,

although within influence of each other, they are able to turn magnetically to the north. In this, relatively to each other, neutral position, they will repulse each other; but if the needle be moved northwards, or southwards, to A'' , for example, then the difference between attraction and repulsion is in favour of the former, and the needle commences to move towards the south end of the bar. If in its progress thence it meet with a constraining guide, g, g' , without friction the needle would float towards the neutral point, and at whatever distance southwards on that guide the needle might be placed, it would, if not too distant, still float up to it. If parallel to the axis of the bar, another guide were placed like the first, a second needle, placed in it, would float towards the neutral point, like the first, and other needles, at other distances, would all float in a similar manner into the same neutral line.

Now, the sun is a great magnet; the earth is a magnet, the planets and satellites, &c., are magnets. Each has its axis lying north and south. Each exerts its magnetic influence more or less upon each of the others, but the predominating influence is, of course, that of the sun, and around him then would be a neutral plane, into which any planet, being north or south of it, would float backwards and forwards with a vibratory motion.

The only difficulty that I see in this theory is that the sun's equator has an inclination of nearly 8° to the ecliptic, but that may possibly admit of explanation. The theory, as a suggestion, may be worth attention.

As the sun, the earth, the planets, the satellites, the asteroids, are each a magnet, or magnetised body, who can say that every tiny atom of nebulous matter in vast space is not the same, and capable of obeying the impulse of nature as if it were a world in itself, for in nature quantity, time, and space, are as nothing? And do we not indeed

see in the rings of Saturn an illustration of it? Each atom of that ring, by its own gravitation and its own eastation, circles in its own orbit of equilibrium round its primary, whilst the great Saturnian magnet keeps it in the plane of the ecliptic, preventing its wandering to the north or to the south without returning again to it.

A wide difference must be drawn between the nature of the energy of eastation proper, and that of the momentum derived from it. . .

The force of eastation at the time of its birth cannot act with centrifugal effect, because its direction at that instant is not in opposition to gravitation, but perpendicular to it; but it takes full effect on the body by imparting to it a motion equivalent to what is lost at the same instant through the negative work before referred to. At its birth it takes possession of the body with a definite direction, and is compounded with other forces; and at every instant an impulse of eastation, *in a new direction*, is given, only to become momentum in the next instant. The momentum is at once called upon to do its work, at first in a degree so infinitely small as to be certainly not sensible to us, but increasing until it be expended, and, at least so far as we are able to trace it, annihilated. At each moment of time then, centrifugal force is created, not by any direct impulse of eastation, but by a force compounded out of the remaining energy of an infinitely large number of previously given impulses of it; and the proportion expending of an impulse, or of the available remainder of one, will increase whilst such available remainder diminishes.

By the constant resuscitation of this momentum, then, a planet is prevented from falling to its primary, and is upheld as effectually as if it were resting upon a substantial pillar, and enabled to return, again and again, with undiminished velocity, to the same point in its orbit through which it had,

at some previous time, passed, and that, too, in a manner that requires neither obscure mathematical manipulation nor doubtful premises for its demonstration, but admits of explanation in perfect accord with our adopted laws of motion. The primary, round which a planet revolves, has properties of a similar character; and, as the direction of the energy of eastation in both is always perpendicular to the line connecting the two bodies, a concurrency in the opposite and parallel motion is secured and maintained.

With the energy of eastation as an accepted fact, it becomes tempting to indulge one's imagination in theoretical and fanciful creations.

Supposing nebulous matter to be constituted of elementary particles possessing different powers of eastation, and capable of separation, from each other, each particle of that matter would move in an orbit of its own under the joint forces of gravitation and eastation round the centre of mass, and the orbit would contract by the insufficiency of the accumulation of momentum to meet gravitation until reaching the position of equilibrium fixed by the constitution of its forces. Particles with similar energy would take similar positions, and form themselves by aggregation into rings round the centre of gravity; such a ring might, without disturbance, revolve for a very long time, but its constitution would alter so soon as the aggregation of particles became sufficiently dense for them to collect, by inter-mutual attraction into masses, small or large, which masses would continue to revolve in the same orbit, and with unaltered velocity round the primary.

In this fashion the rings of Saturn may be considered to be formed, but the quantity of the gatherings from space seem, each, in its kind, to have been comparatively small, and they have been brought into very narrow limits by mutual attraction, whilst kept in one plane by the Saturnian

magnetic influence ; they are extremely thin when compared with their depth, being about 100 miles thick, and 14,800 miles deep, the inner edge being only about 20,000 from the surface of the planet. The thinness of the rings may account for the inter-attraction of the particles having failed hitherto to break the equilibrium of the component parts, but that is no reason that we might not, at any day, behold the spectacle of the shattering of one of the rings, and its transformation into a sphere and a satellite. Such an exhibition would only be one of the natural phenomena which may have been exhibited, though, perhaps, not witnessed by man, at the formation into a sphere of each of our planetary bodies.

The period of the ring of Saturn is thought to be the same as that of the primary, because dark markings on it have been seen to revolve with it. That supposition, however, seems to be against nature ; and I imagine the markings observed must have been the effect of light radiated or reflected from Saturn himself, or of some physical condition on his surface. The period of the rings, if revolving with Saturn, would complete their period in $10\frac{1}{2}$ hours, a velocity that would yield centrifugal force sufficient to shatter and sweep outwards a solid material of any imaginable strength. At the mean distance of the rings from the planet, the velocity necessary to equilibrate gravitation would give a period of about 16 hours. In the face of the inevitably enormous centrifugal effect, it is difficult to conceive how any one can, for a moment, tolerate the idea of the rings having a period equal to Saturn himself.

If the substance of the rings be fluid, and they cannot but be so, every particle of it, free to move, must move in virtue of its own independent properties, and it seems probable that an enormous amount of friction, with all its consequences, must occur in their doing so.

The shortening of the axial period of a sphere, with its

distance from its primary, is quite in accordance with my theory that a comparatively greater energy of eastation is the cause of a planet's orbit being enlarged. At the same time it is not imperative that there be no apparent deviation from this rule. For example, the rotation of the earth performs a certain amount of work, which must give her a longer axial period than she would have without such work ; but that would not interfere with her power of progression in her orbit. Again, in Mars there is evidence of the existence of large masses of water, similar to those on the earth, and if we imagined the proportions, or the physical conditions, of the land and water there, to be such as to be conducive of more work than is required on our earth, the brake will be more severely felt, and we have, at once, a perfectly simple explanation of how Mars may, with a greater energy of eastation than the earth, have a longer period of rotation.

Whilst Mars is half as far again as the earth from the sun, yet he takes half-an-hour longer to rotate on his axis.

After imagining the rupture of a planetary (or solar) ring, we can further imagine it to contract and form into a sphere. Such sphere would, by its energy of eastation, commence an axial rotation. The period would at first be long, but it would shorten, until the expenditure of momentum in the production of circulation of the fluid parts and in other work equalled the supply from eastation, when an equilibrium would be found, and the period remain invariable. The theory has an attractive simplicity about it.

It is said that all rules have exceptions, and so it is in this case. Within our observation, there is our moon forming a noteworthy exception to the rule laid down. The same hemisphere is always turned by her towards the earth. In her progress eastwards the same hemisphere is always kept in advance, like the head of an arrow. The generally received theory of this is that, like all celestial bodies, she

has been placed in her orbit by the Great Creator with an *initial impulse* exactly suited to keep her in that orbit, and that, also, at the time of being launched she received a slight twist on her axis, so exactly adjusted to her orbital period that their times have remained equal and concurrent ever since.

"The extraordinary relation existing between the moon's period in her orbit and the time occupied in her axial rotation gives us the opportunity of ascertaining whether our system has received any external shocks. These two periods are so accurately adjusted, that in all respects an equality exists. The moon ever turns the same hemisphere to the earth, and ever will, unless some external cause should arise to disturb the perfect harmony which now reigns. It is not my purpose to explain why it is that the phenomenon exists. I merely desire to state, that the delicate balancing of periods furnishes an admirable evidence that, for several thousands of years, at least, no shock has been received by the earth and its satellite." (Professor Mitchell's "Planetary and Stellar Worlds.")

A very simple explanation of the phenomenon may be hazarded by the theory of *eastation*. Let us suppose that at one time, long ago, the earth possessed a solidified moon, half the size of the present one, but between the spheres there floated a ring of matter, such as we at present see around Saturn. When the time of the springing of the ring and the formation of a sphere came, it might happen that by the jerk of the spring, or by too close a proximity to the moon, the momentum of the newly-formed sphere were insufficient to withstand the attraction to the moon, in which case the two spheres—one solid, one fluid, would collide into one. Separately, the two masses possessed different powers of eastation, and that mass which possessed the superior power would, *at once*, take a leading position to the east (relatively to its primary), dragging the other after it with a velocity compounded out of the two (and other) forces—a velocity slower than what one mass, faster than what the other, would, separately, have possessed. Unless the whole

substance of the composite moon became homogeneous by the dissolution of the older solid sphere, that portion of it having the greater energy of eastation, would maintain its leading position eastwards, in spite of the rotatory energy of the whole, especially when we consider that no axial momentum could, at the time, have been created.

With energy of eastation, the most attenuated nebulous matter may, in space, move with the same velocity as a dense body, and in a similar orbit, and it may acquire comparatively equal momentum.

It would be not a small triumph for the invention of the theory of eastation, if, by it, the extraordinary phenomena of "comets' tails" could be explained.

The time for its consideration now is very *à propos*, following so soon after the appearance of one of our august cometic visitors.

In the September number of the *Cornhill Magazine* a consideration of "the more promising attempts which men of science have made to solve the mystery of comets' tails," has been presented to the public. The writer looks with more favour upon the "repulsion theory" than upon any other, and the article is concluded with these pregnant words :—

"We may still say, as Sir John Herschel long since said,—'There is, beyond question, some profound secret and mystery of nature, concerned in the phenomena of comets' tails. Perhaps it is not too much to hope that future observation, borrowing every aid from rational speculation, grounded on physical science generally (especially those branches of it which relate to the ethereal or imponderable elements), may ere long enable us to penetrate this mystery, and to declare whether it is really matter, in the ordinary acceptation of the term, which is projected from their head with such extravagant velocity, and if not impelled, at least directed in its course by a reference to the sun as its point of avoidance.'"

I have already supposed that the constituent parts of nebulous matter, in space, possess different powers of easta-

tion, and that they are capable of filtering down, and collecting, like with like, or approximately so, in orbits round centres of mass. The writer in *Cornhill*, from observed phenomena, drew conclusions of a similar character, but in respect of the sun's repulsive power on different particles.

The nearest fixed star is a point of attraction—a centre of gravitation like our own sun. Between that star and our sun there is a line where the attractive force of each equilibrates that of the other. On each side of that line, respectively, nebulous matter would sink towards its primary, each particle having a prospect of reaching its orbit *in the course of time*. That time would, however, vary widely—very widely, and although a large proportion of it has probably by this time become consolidated into dense spheres, quantities must be still *in transitu*, and liable to be gathered in by the attractive property of any passing body. Until finding its orbit, the constituent parts of the nebulous matter would remain intermixed, and a comet passing, from any cause whatever (and we are at liberty to take the fact as we find it, like Newton took centrifugal force in each moment of time, without inquiring into causes)—a comet passing downwards through this nebulous matter would gather quantities of it together. In bodies meeting under such circumstances the impact probably would not be so violent as to produce combustion, nor the mass sufficient to produce heat by compression. For similar reasons I think the meteoric stones which first reach our atmosphere as falling stars can only have previously existed as nebulae, and become ignited through friction in our atmosphere. I have shown that by the energy of eastation a nebulous cloud might travel in vacant space with all the velocity and all the momentum that it could have acquired if previously compressed into a solid sphere.

A cometary nucleus, at a vast distance from the sun, and

with little attraction of its own, and moving at an easy pace through a nebulous cloud, would gather it in with but slight impact and friction, insufficient perhaps to destroy the individuality of the particles. Possessing elasticity, the matter might be compressed, and go to increase the size of the nucleus, without losing their individuality. So long as the feeble intermutual attraction of the constituent particles be not overcome by stronger forces, the whole will hold together as a denser mass, and all accumulated energy (momentum) will become a common property; yet in case circumstances should arise, by which the centrifugal energy of one particle be increased more than that of others, to such an extent as to overcome the opposing force of intermutual gravitation, that particle will, by its own energy, move away from the others; and if it could, by overcoming friction, disengage itself from the others, it would, in space, possess all the physical properties of a dense planet having a greater energy of eastation, and, like one, move by its superior momentum and centrifugal force away from others, as if by an act of repulsion. So far then I am justified, in assuming an analogy between the planetary bodies and the individual constituent parts of the cloud which will now be following the cometary nucleus. Instead of following an aggregation of particles into the vicinity of the sun, let us select two of them, and magnify them, for facility of observation, into spheres.

Before proceeding, however, I must digress for the purpose of acquainting my readers with some peculiar phenomena observed in comets, and exhibiting the operation of some overwhelming power, which, by the writer in *Cornhill*, has been attributed to a repulsive property vested in the sun; that writer says—

“But there is something so startling in the conception of a repulsive energy competent to account for the formation of comets’ tails,

that one naturally seeks for any explanation which may account for the phenomena without forcing upon us the idea of so amazing a force. Especially is this the case when we consider that, on the theory of repulsion, the old tails, enormous though their dimensions are, must be regarded as continually dissipated into space ; so that we have to suppose a series of tails, each many millions of miles in length, and of vast breadth and thickness, all formed from out of the substance of the comet, and swept for ever away from it. It is easy indeed to speak of the retreating comet gathering its substance together when once beyond the domain of the *sun's repulsive power*, but the velocity with which that substance is swept away is such as not even the sun himself could overcome by his attractive energy : much less could the feebly attracting head of a comet draw back the stragglers which the *sun's repulsion* had (on this theory) hurried away into surrounding space."

Clearly no suspicion crossed the mind here, that the tail could sweep itself away by its own energy, simply because we have been taught to believe that the comet and its tail move by no energy but that of the momentum of an initial impulse and gravitation, the nucleus and the tail possessing like energy from that impulse. The only conclusion that could possibly be come to in the face of "positive evidence" of an effect resembling that which would be produced by repulsion, was that the sun possessed a "*repulsive power*," but the idea of "so amazing a force" is too "startling" to be adopted without the reservation and the hope expressed in the words of Sir John Herschel, and repeated in *Cornhill*.

Centrifugal effect varies *as the square of the velocity*. Some idea of the effect of this may be gained when we consider that, on the equator of our earth, a body is, by the velocity of rotation, lifted upwards with but $\frac{1}{189}$ th of the force of gravitation downwards, yet with the velocity increased to only seventeen times what it now is, gravity would be equilibrated, and with anything over that velocity a body (a man, for example) would move away, upwards, producing an

effect that might be regarded as that of a "repulsive" property of the earth. This imaginary man would, according to present theories, have started on a long journey; he would share the fate of Comstick's cannon-ball, and become a revolving satellite of the earth, never again to return to it. It may seem singular that, by the energy derived from a single blast of powder, a cannon ball may, theoretically, be turned into a satellite, and be able to stand for ever and ever, the constant strain of gravitation. It may seem singular: one would think that the energy of the explosion could be capable of sustaining the weight of the ball only for a limited time; but Newton has led people into believing that it could sustain it for ever and ever, and mathematical manipulation has appeared to confirm the truth of it.

We are now ready to again follow the two spheres, the representatives of a comet and a particle of its tail, adhering together by mutual attraction, and approaching the sun with an accelerative velocity. The representative of the tail possesses a greater energy of eastation than the other, so I will call it *F* (faster), the other *S*, because slower. In obeying gravitation, the acceleration in both is equal, but in making easting *F* will draw *S* after it, and impart to it more momentum than, alone, it would have had.

As the sun is approached, the velocity from eastation increases inversely almost as the square of the distance from it; *F* and *S* generating, in different degrees, each its own energy; consequently the differential energy will increase rapidly, and will act in direct opposition to the attraction. There will come a moment when the differential energy is equal to and more than the attraction, and the two spheres will then commence to move asunder *as if* by a repulsive force. This is the critical moment; let us watch carefully.

Momentum is rapidly expended, but more rapidly compensated ; in F it is compensated more rapidly than in S , and the velocity of F will relatively increase whilst that of S diminishes ; and as centrifugal effect increases with the square of the velocity we have at once a cause, mighty yet simple, capable of producing that tremendous centrifugal velocity, which has been called an unknown power of repulsion residing in the sun. Through the sluggishness of S , the sphere F has been constrained to approach the sun much nearer than its own energy of eastation would have allowed it to do, hence, it would, when freed, shoot off with so much more vigour.

Now if we consider the nebulous cloud gathered round the nucleus of the comet to consist of an infinite number of particles, each capable of exerting its own special power of eastation and centrifugal force when set free ; we have all the elements for a shoal or stream of planetary atoms, each of which is disengaging itself by its own force from an orbit into which it had been forced by fortuitous circumstances. Any particles of the cometary "tail," having a lesser energy of eastation than the mean energy of the nucleus, will fall off the other way into an inner orbit upon being disengaged, and thus, that most perplexing of all features of comets, the two tails, one towards the sun, the other from it, may be explained in an easy manner, by the simple mechanical combinations of gravitation and eastation.

A very singular feature in Donati's comet is described in *Cornhill* :—

"So far, then, there was no special reason for preferring either theory. But now a circumstance was noted, which, so far as we can see, the *repulsion* theory is alone competent to explain. We must note that the reasoning which follows, though it presented itself independently to the present writer, was long before adduced by Professor Norton, of Yale College, in America, the well-known author

of the auroral theory of the solar corona. The great mass of the matter undergoing *repulsion* was carried into the large, bright, curved tail. We can conceive that in thus moving off, this matter, being so much greater in quantity, would be apt to carry off along with it, and, as it were, entangled in its substance, portions of the matter which should have gone into the small tail—the matter, namely, on which the *sun's repulsive action* was able to act more swiftly, sweeping it out into straight lines. The matter thus carried away into the wrong tail, as it were, would be always ready to escape from the entanglement so soon as the matter which had carried it off began, through wide-spreading, to leave it free. And thus at once the *sun's repulsive action* would act upon this matter precisely as on the matter of the same kind forming the straight tail; it would *repel* this matter, which had escaped from the entangling matter of the curved tail, and sweep it away in a straight line, so that it would form, as it were, a sort of subsidiary tail, not extending from the head, but from a particular part of the curved tail. This happening from time to time, the curved tail would manifestly have a number of straight tails, or streamers, all extending on the same side of it as the straight tail which streamed from the head. Now, this was precisely the appearance presented by the curved tail of Donati's comet—a sort of combing out, or striation, the direction of the different streamers corresponding exactly with that which would result from the mode of formation just described."

The supposition that the substance of the tail is composed of particles possessing different physical properties in regard to the *sun's power of repulsion*, is similar to my supposition that nebulous matter is composed of particles having differing powers of eastation, and the liberation of sets of particles in puffs, by disentanglement, is compatible with either theory.

On a large scale, we see in the sun a burst of flame 20,000, 30,000, or 40,000 miles high; on a small scale we see steam collect into bubbles before rising from the heating surface. In both these cases liberation might, one would think, have taken place gradually instead of in bursts, but it does not.

A palpable difficulty in "the sun's repulsion" theory, may be demonstrated.

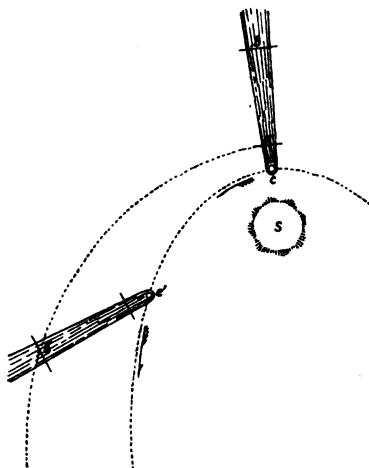


Fig. 15.

In Fig. 15, let S be the sun, C and C' the comet at different points in its orbit.

When the comet is at C , we will observe a particle of the tail situated at " a ." If the comet were stationary the particle would move under the sun's repulsion, from a to b in a given time; but in nature, the comet moves in the same given time from c to c' , and the particle by the repulsion, from a to b' . In this particular part of

the orbit, the velocity of the comet is diminishing, likewise therefore, should be that of the particle at a . If, now, the comet at c , and the particle at a , move only under the energy of a primitive impulse, whence comes the additional *angular* force which impels the particle with such a vast accelerative velocity from a to b' , whilst the nucleus with a diminishing velocity, moves from c to c' —only about half the distance?

The reasonable reply (if the ghost of Newton do not rise up against it) is that the particle moves under its own independent and constant energy of eastation, that it has been brought by the attraction of the nucleus into an abnormal proximity to the sun, and that as soon as liberated, its surplus energy, after meeting the sun's attraction, accumulates

into an increasing momentum : increasing its onward velocity, and its centrifugal force increasing *as the square* of its velocity, and so producing the movement which has been ascribed to an incredibly enormous repulsive property vested in the sun.

One more consideration, and I will dismiss this very interesting subject.

Space is filled with æther, said to be almost infinitely attenuated, as doubtless it is. As a substance, we cannot but consider it to be material, because it vibrates under the action of light, and as a material substance, it must be, to some extent, however small, a *resisting* medium.

Through this ether the comet's tail moves with more than planetary velocity ; but it is questionable which vapour is the more attenuated—the ether or the tail.

What does Professor Tyndall say about the tenuity of the tail ? He says :—

“ As regards tenuity, I entertain a strong persuasion that out of a few ounces (the possible weight assigned by Sir John Herschel to certain comets) of iodide of allyl vapour, an actinic cloud of the magnitude and luminousness of Donati's comet might be manufactured.”

Let any one imagine a tenuous vapour of this kind, moving under an *impulse only*, from the point *a* in the last figure as if shot from the mouth of a cannon, and think how far it can be possible for that vapour, in virtue of the force of the impulse, to perform, as it does in nature, the path to *b* through the æther medium, however much attenuated that medium may be.

I hope that these, my humble speculative inquiries into the mechanical production of centrifugal effect, will lead to further inquiry into the subject by men whose special province it is to do so. A more important subject it would be difficult to find. I confess that, before getting well into my

subject, I almost despaired of making a case strong enough to overcome the prejudice of preconceptions sufficiently to induce others to re-examine Newton's magnificent work, and judge for themselves whether the casually expressed words that gravity "abundantly serves to account for all the motions of the celestial bodies, and of the sea," should be read in a literal sense, or in connection with the context, and with his work itself.

Even Professor Mitchell admits that—

"A broad and distinct line must be drawn between those phenomena for which gravitation must render a satisfactory account, and those other phenomena for which it is nowise responsible. In the solar system we find, for example, that all the planets revolve in the same direction around the sun, in orbits slightly elliptical, and in planes but little inclined to each other. Neither of these three peculiarities is in any way traceable to the laws of gravitation."

For myself, I have no fear nor hesitation in asserting that against the elastic pull of gravitation, neither the oblateness of a planet, nor the integrity of its orbit, could be maintained by an initial impulse only, as Newton is said to have demonstrated that it could.

THE END.



